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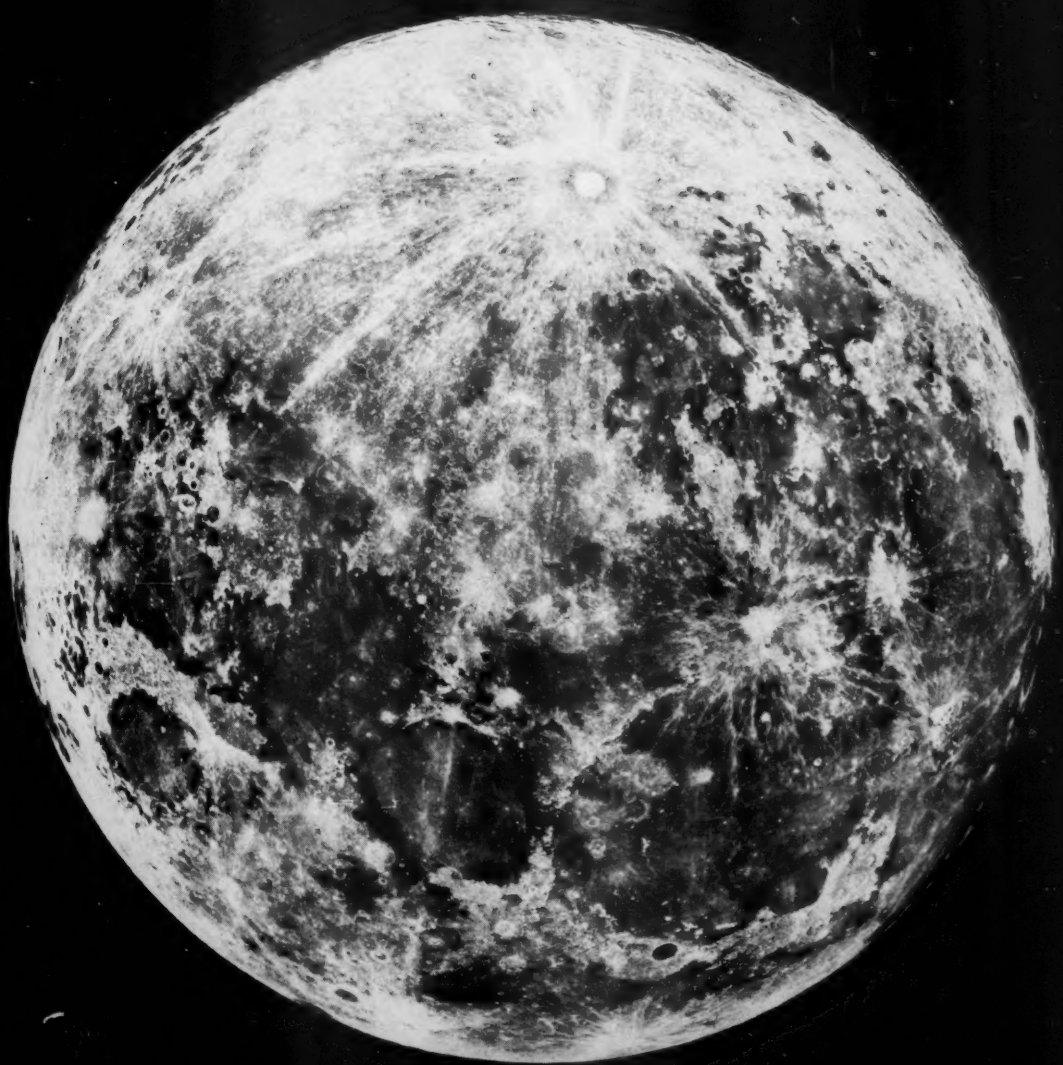

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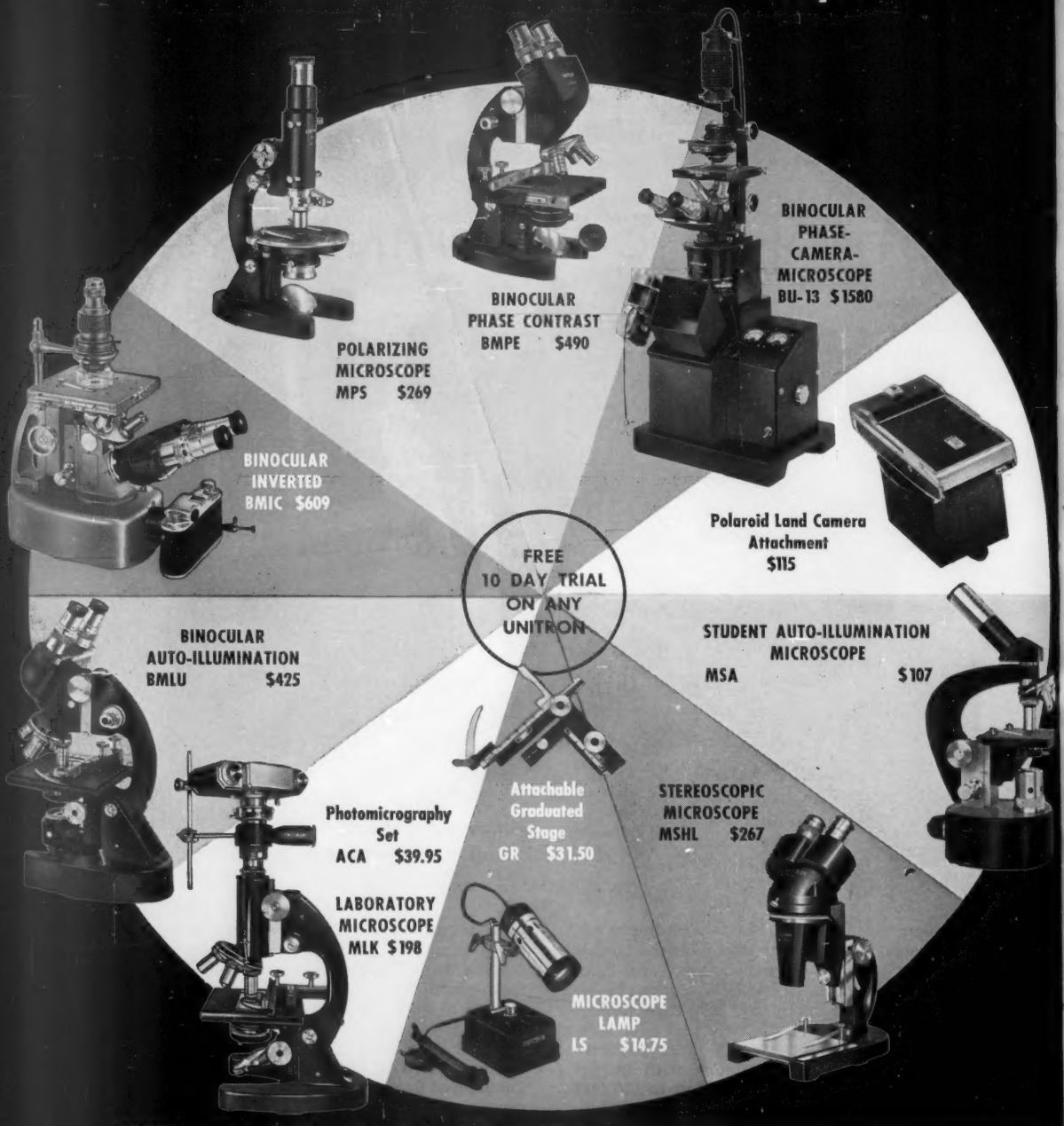
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Cover

The full moon as photographed with the 36-inch telescope of the Lick Observatory on 17 January 1946. The dark *maria* (almost certainly lava fields) and bright uplands or *terrae* are plainly visible. Most lunar features, such as craters, can be seen only under oblique illumination. Parts of the uplands are apparently the original lunar crust, modified locally by impacts, while other parts are covered with rubble ejected from nearby *mare* basins or large impact craters. [Photograph is plate 1 of the *Photographic Lunar Atlas*, published by the University of Chicago Press; see page 290]

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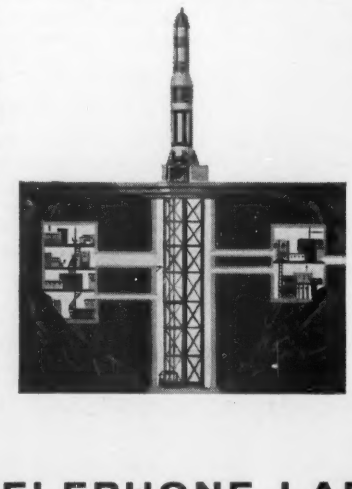

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Science Ambassadors

Early in 1958 the Department of State reactivated the program of assigning science attachés to U.S. embassies in other countries. Wallace Brode was appointed science adviser to the Secretary of State, and scientific representatives have since been sent to 10 major capitals abroad. We recently had the opportunity to discuss the program with a few science attachés and with several scientists in Europe who have observed the work of our scientific ambassadors.

There have been some initial problems. A new science attaché must demonstrate that he can live and work as a member of the embassy staff and that he can contribute to the working effectiveness of the embassy. In the 18 months since the first attachés reached their posts, much of their time has gone into learning how to work most usefully. Now, the report is, "We are beginning to be truly effective." They facilitate scientific exchanges with the U.S. They interpret American science to scientists, science writers, editors, and others who influence foreign attitudes toward the U.S. and its scientific and technological achievements. And—as a function of prime importance to the Department of State—they help other members of the embassy staffs to give proper consideration to the scientific and technical factors involved in the decisions they must make. The program is well started; the attachés express a feeling of growing accomplishment in what they are doing.

What of the future? The program is, in a sense, on trial. Before it can work at maximum effectiveness, three hurdles must be crossed. First, the Department of State must demonstrate that it gives the program continuing, nonpolitical support. Both the present administration and the administration that takes office on 20 January must be alert to the importance of demonstrating the continuing, nonpolitical character of the program.

The second hurdle is to persuade first-rate men to replace the present attachés, most of whom were appointed for two-year periods while on leave of absence from their permanent positions. If the Department of State passes the first hurdle successfully, the science adviser can recruit good replacements; if the Department fails the first hurdle, the second will also surely be failed.

The third hurdle is to work out long-term staffing policies. The well-selected amateurs in diplomacy we now use have dedication, knowledge of American scientific activities, and considerable acquaintance with the language, customs, and scientific activities of the countries in which they work. Ideally, they should also have a greater understanding of national policy and of Department of State procedures and problems. It is possible to combine the advantages of an amateur with those of a professional, perhaps most effectively by making periodic foreign service a recognized part of the career patterns of appropriate scientists and science administrators, but it will be a waste of time to worry about this third hurdle unless the first one is successfully crossed.

Our emphasis on the first hurdle is because the Department of State failed on this one once before. In 1951 the Department appointed a science adviser and a number of science attachés. When the science adviser resigned, no replacement was appointed; as the attachés came home, their posts were left vacant. Thus the program stumbled to a halt in 1956. The service that the program can render to the nation is too important to allow the first hurdle to be failed again.—D.W.

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CURRENT PROBLEMS IN RESEARCH

Elementary Particles of Modern Physics

The properties of the 30 particles and antiparticles, their forces and conservation laws are summarized.

R. E. Marshak

In popular usage, the term *elementary particle* signifies an ultimate constituent out of which all matter is compounded. In physics, the usefulness of the concept depends very much on our state of knowledge, the hierarchy of forces with which one is dealing, and the order which is introduced into the description of the empirical facts.

From one point of view, any particle with a well-defined mass, charge, and intrinsic angular momentum (or spin) is an elementary particle and, in this context, even a molecule could be regarded as elementary. However, when the electromagnetic law of force was established between the atoms in a molecule and also between the electrons and the positively charged nucleus of the atom, it was much more convenient at that stage to think of the electron and the various types of atomic nuclei as the elementary particles. When quantum mechanics was developed and the wave-particle dualism became an essential ingredient of our understanding of all atomic phenomena, it was proper to add the photon to the list of elementary particles. When the neutron was discovered and the existence of a distinct nuclear force was established, it became more advantageous to think of the neutron and proton as the elementary particles out of which atomic nuclei are built up. Within this context, for example, the completely stable deuteron, with a well-defined mass, charge, and spin, is considered a composite structure, whereas the unstable neutron is treated as an elementary particle. The reason for this anachronistic point of view is that within the hierarchy of strong (or nuclear), electromagnetic, and weak forces, the neutron lives for a very long time (10^3 seconds) on the nuclear time scale of 10^{-23} second.

The situation is even more curious. If we restrict ourselves to low-energy nuclear phenomena, the only elementary particles which enter the picture are the electron, positron, proton, neutron, photon, neutrino, and antineutrino. When some of these particles acquire sufficient kinetic energy (either as components of the cosmic radiation or in high-energy accelerators), many new varieties of particles are created which, at this stage of our understanding, are added to the list of elementary particles. Some of these particles, which result from the conversion of kinetic energy into mass, in turn create new particles through decay or through secondary production.

As a result, the following additional particles (and antiparticles) have been observed and must be added to the list of elementary particles: the positive and negative muons; the positive, negative, and neutral pions; the positive and negative kaons and two kinds of neutral kaons; the antiproton and the antineutron; the neutral Λ-hyperon and the anti-Λ-hyperon; the positive, negative, and neutral Σ-hyperons; and the negative and neutral Ξ-hyperons. For reasons which will become apparent, it is quite certain that the three anti-Σ-hyperons and the two anti-Ξ-hyperons will be observed when the proper experiments are performed (*1*). We thus end up with 30 particles and antiparticles, of which seven (the photon, neutrino, antineutrino, electron, positron, proton, and antiproton) are genuinely stable. The remaining 23 particles and antiparticles possess such a slight degree of instability (on the nuclear time scale) that we are justified in treating them on an equal footing with the seven genuinely stable particles and antiparticles.

Properties

The properties of an elementary particle which can be directly measured are the charge, mass, spin, and, if the particle is unstable, the lifetime. These properties are listed for all 30 particles and antiparticles in Table 1. The charges, masses, spins, and lifetimes of the antiparticles are not listed separately; the reason for this is that if the laws of special relativity apply to all physical processes involving the elementary particles, as we believe they do, it can be shown that the charge of the antiparticle must be the negative of that of the particle, and that the mass, spin, and lifetime of the antiparticle must all be identical with those of the particle. The neutral kaon is an exception to this statement where we have listed two distinct lifetimes for two particles which are called the K_1^0 and K_2^0 particles; the K_1^0 and K_2^0 particles are opposite mixtures of the neutral kaon and its antiparticle, and the exception occurs because the neutral kaon and its antiparticle can convert into each other through the weak force.

The author is Harris professor of physics at the University of Rochester, Rochester, N.Y. This article is adapted from a lecture delivered 27 December 1959 at the Chicago meeting of the AAAS.

It will also be noted that antiparticles for the neutral pion and the photon are placed in parentheses; this is because there is no known property by means of which the antineutral pion and the antiphoton could be distinguished from the respective particles. This also explains why the list in Table 1 contains 16 particles and 14 antiparticles; of these, four particles (proton, neutrino, electron, photon) and three antiparticles (antineutrino, positron, antiproton) are stable and the remaining 12 particles and 11 antiparticles are unstable.

Mass and Charge

The particles in Table 1 have been ordered chiefly according to their mass; the one ambiguity, between the photon and the neutrino, has been resolved by placing the photon first on the list. This mass ordering leads to a remarkable grouping of the particles into four classes: photon, lepton, meson, and baryon. The names *lepton, meson,* and *baryon* were originally invented to denote light, intermediate-mass, and heavy particles, respectively, but we now know that the particles in each of these three classes have certain important properties in common which are distinct from the properties characterizing the members of the other two classes.

If we further examine Table 1, we find that the (electric) charge number Q for the particle (the Q for the antiparticle is the negative of the Q for the particle takes on the values 1, 0, and -1 (e being the fundamental unit) and that for no elementary particle is $|Q| > 1$. Also, the range of masses of the particles listed in Table 1 is not completely arbitrary; there appear to be some important mass groupings, within at least the meson and baryon classes, of particles which bear a direct relationship to the charge numbers. Within the meson class, the charged pion is only 4.6 Mev heavier than the neutral pion, and the neutral kaon is only 3.9 Mev heavier than the charged kaon. In the baryon class, the neutral nucleon (that is, the netutron) is only 1.3 Mev heavier than the charged nucleon (the proton); the negatively charged Σ-hyperon is only 6.8 Mev heavier than the positively charged Σ-hyperon; this, in turn, is only 2.4 Mev lighter than the neutral Σ-hyperon; and the masses of the negative and neutral Ξ-hyperons are within 8 Mev of each other (this is experimental error). By the same token, it may be meaningful to consider the neutrino and electron, which differ in mass by only 0.5 Mev, as the neutral and negative "light" leptons in contradistinction to the "heavy" lepton, which is the muon. These mass differences are all small compared to the mass differences between the average masses of the "light" and "heavy" leptons (within the lepton class), between the average masses of the pion and kaon (within the meson class), and between the average masses of the nucleon, Λ-hyperon, Σ-hyperon, and Ξ-hyperon (within the baryon class).

It seems, therefore, that in some sense the particles which differ only slightly in mass are different charge states of the same particles, so that the distinct elementary particles whose properties and interactions we must understand are the photon, the "light" lepton, the "heavy" lepton, the pion, the kaon, the nucleon, the Λ-, Σ-, and Ξ-hyperons, and all the antiparticles of these particles. The slightly different masses associated with the different charge states of each of the above particles are then attributed to electromagnetic self-energy effects produced by virtual interactions with the electromagnetic field. While no quantitative theory of these mass differences exists, the electromagnetic approach appears plausible and involves no difficulty of principle except possibly for the "light" lepton.

Spin

The regularities among the spins of the particles listed in Table 1 are even more striking than the regularities in the charge and the mass. Except for the photon, which is in a class by itself and possesses spin 1 (in units of $\hbar$, Planck's constant divided by 2π), all

Table 1. Properties of elementary particles. Masses and mean lifetimes are taken from the Berkeley compilations.

	Particle	Anti-particle	Spin	Mass (Mev)	Charge No. Q	Baryon No. B	Lepton No. L	Strangeness No. S	Mean life (sec)	Decay modes
				Photon						
Photon	γ	(γ)	1	0	0	0	0	0	Stable	
				Leptons						
Neutrino	ν	$\bar{\nu}$	½	0	0	0	+1	0	Stable	
Electron	e^-	e^+	½	0.510976 ± 0.000007	−1	0	+1	0	Stable	
Muon	μ^-	μ^+	½	105.70 ± 0.06	−1	0	+1	−1	$(2.261 \pm 0.007) \times 10^{-6}$	$e^- + \nu + \bar{\nu}$
				Mesons						
Pion	π^+	π^-	0	139.63 ± 0.06	+1	0	0	0	$(2.56 \pm 0.05) \times 10^{-8}$	$\mu^+ + \nu$; $e^+ + \nu$
Pion	π^0	(π^0)	0	135.04 ± 0.16	0	0	0	0	$< 4 \times 10^{-16}$	2γ
Kaon	K^+	K^-	0	494.0 ± 0.2	+1	0	0	+1	$(1.224 \pm 0.013) \times 10^{-8}$	$\mu^+ + \nu$; $\mu^+ + \nu + \pi^0$; $e^+ + \nu + \pi^0$ $\pi^+ + \pi^0$; $2\pi^+ + \pi^-$; $\pi^+ + 2\pi^0$
Kaon	K^0	$\bar{K}^0$	0	497.9 ± 0.6	0	0	0	+1	K_1^0: $(1.00 \pm 0.038) \times 10^{-10}$	$\pi^+ + \pi^-$; $2\pi^0$
									K_2^0: $(6.1 \pm 1.3) \times 10^{-8}$	$\pi^+ + \pi^- + \pi^0$; $3\pi^0$ $\mu^+ + \nu + \pi^-$; $\mu^- + \bar{\nu} + \pi^+$ $e^+ + \nu + \pi^-$; $e^- + \bar{\nu} + \pi^+$
				Baryons						
Nucleon	p	$\bar{p}$	½	938.213 ± 0.01	+1	+1	0	0	Stable	
Nucleon	n	$\bar{n}$	½	939.506 ± 0.01	0	+1	0	0	$(1.04 \pm 0.13) \times 10^3$	$p + e^- + \bar{\nu}$
Λ-Hyperon	Λ	$\bar{\Lambda}$	½	1115.45 ± 0.12	0	+1	0	−1	$(2.505 \pm 0.086) \times 10^{-10}$	$p + \pi^-$; $n + \pi^0$ $p + e^- + \bar{\nu}$
Σ-Hyperon	Σ^+	$\bar{\Sigma}^+$	½	1189.55 ± 0.21	+1	+1	0	−1	$(0.84 \pm 0.07) \times 10^{-10}$	$p + \pi^0$; $n + \pi^+$
Σ-Hyperon	Σ^-	$\bar{\Sigma}^-$	½	1196.39 ± 0.34	−1	+1	0	−1	$(1.59 \pm 0.10) \times 10^{-10}$	$n + \pi^-$
Σ-Hyperon	Σ^0	$\bar{\Sigma}^0$	½	1191.94 ± 0.53	0	+1	0	−1	$< 0.1 \times 10^{-10}$	$\Lambda + \gamma$
Ξ-Hyperon	Ξ^-	$\bar{\Xi}^-$	?	1319.1 ± 0.5	−1	+1	0	−2	$(1.9 \pm 0.5)\ 10^{-10}$	$\Lambda + \pi^-$
Ξ-Hyperon	Ξ^0	$\bar{\Xi}^0$	?	1311 ± 8	0	+1	0	−2	1.5×10^{-10} (1 event)	$\Lambda + \pi^0$

the particles in the meson class possess spin zero, whereas all the particles in the lepton and baryon classes possess spin 1/2. (The spin of the Ξ-hyperon has not been measured as yet, but it is rather likely that this will also turn out to be 1/2.) Thus, all the elementary particles (with the exception of the photon) possess the lowest half-integral (1/2) or integral (0) spins that are possible.

Quantum Numbers B and L

We have also listed in Table 1 the values of two quantum numbers—the baryon number B and the lepton number L—which have been assigned to each elementary particle (B and L for the antiparticles are discussed below). The introduction of these quantum numbers and the imposition of conservation laws involving them help greatly to reduce the multiplicity of processes in which the elementary particles might otherwise participate. It is to be hoped that the future fundamental theory will provide a natural basis for these quantum numbers, but at this stage they must be regarded as essentially having a phenomenological origin. The baryon and lepton quantum numbers have already been anticipated in the names assigned to two of the classes of elementary particles: each member of the baryon class is assigned $B = 1$ (the antibaryon is assigned $B = -1$), while the particles (and the antiparticles) of the other three classes—photon, lepton, and meson—are assigned $B = 0$. The particles in the lepton class are assigned $L = 1$ (the antiparticles are assigned $L = -1$), whereas the particles of the other three classes—photon, meson, and baryon—are assigned $L = 0$.

The conservation laws for baryon number and lepton number (like the conservation of charge number) are absolute conservation laws which hold for all three types of interaction (strong, electromagnetic, and weak) which operate among the elementary particles. Also, the conservation laws for baryon, lepton, and charge numbers are similar in character in the sense that a quantum number is assigned to the particle and its negative is assigned to the antiparticle and the conservation law is stated in terms of the requirement that the algebraic sum of the values of the quantum number for a system of particles and antiparticles stay constant for any physical process. A quantum number with this property is called an "additive" quantum number, so the charge number, the baryon number, and the lepton number are all "additive" quantum numbers which must be absolutely conserved.

We can now understand why the photon and neutral pion are their own antiparticles whereas the other neutral particles are not. In the case of all but one of the other neutral particles, one of the additive quantum numbers B or L distinguishes between the particle and the antiparticle. Thus, the neutrino and antineutrino are distinguished by the lepton number L, and the neutron, neutral Λ-, Σ-, and Ξ-hyperons and their antiparticles, by the baryon number B. We shall see below that the neutral kaon and neutral antikaon are distinguished by another "additive" quantum number called the strangeness number.

The absolute conservation law for baryon number has immediate consequences for the production and annihilation of antibaryons. For example, it is evident that if in the initial state there are two baryons and no antibaryons present, then the production of one antibaryon in the final state by any physical process whatsoever will necessitate three baryons in the final state. Conversely, if there are one antibaryon and one baryon present in the initial state, then the disappearance (annihilation) of the antibaryon by any physical process whatsoever can only take place if we end up with no baryons. As far as the empirical evidence for the absolute conservation law of baryon number is concerned, the most incisive evidence comes from studying a possible instability of the lightest of the baryons—namely, the proton. If the baryon number is absolutely conserved, there should be no physical process by means of which the proton can decay into lighter particles. Various experiments (*2*) carried out with this purpose in mind have set a lower limit for proton decay by any mode (for example $p \rightarrow 2e^+ + e^-$) of about 10^{23} years. This is a very long lifetime and implies that the conservation law for baryon number, which is responsible for the stability of atomic nuclei, is even more absolute than charge conservation insofar as the experimental evidence is concerned (where a lower limit of 10^{19} years has been found for the reaction $e \rightarrow \nu + \gamma$).

The conservation of lepton number also appears to be an absolute conservation law. The best (albeit rough) check of this conservation law has come from the study of neutrino interactions with atomic nuclei. In accordance with out assignment of lepton number, a neutron should emit an antineutrino with the electron, and a proton in the nucleus should emit a neutrino with the positron. It follows that the neutrinos produced in a nuclear reactor must be antineutrinos (since the fission of heavy nuclei leads to fission fragments with an excess of neutrons, and thus to electron-unstable nuclides), and therefore the process $\bar{\nu} + p \rightarrow e^+ + n$ is possible, whereas the process $\bar{\nu} + Cl^{37} \rightarrow e^- + Ar^{37}$ must be impossible. The cross section for the first process has actually been measured and found to be $11 \pm 4 \times 10^{-44}$ cm^2 (in agreement with the theoretical prediction), whereas the upper limit on the cross section for the second process has been found to be $0.1 \pm 0.6 \times 10^{-45}$ cm^2, at least ten times smaller than would be expected if the neutrino and antineutrino were not distinguishable particles. All the other weak interaction processes involving leptons, including the decays of the strange particles, are consistent with the conservation of the lepton number (within an experimental error of 10 percent).

Stable Particles

There are several points to note in connection with the lifetimes listed in Table 1. First of all, the stable particles are the photon, the two "light" leptons (neutrino and electron), and the lightest of the baryons (that is, the proton) and their antiparticles. The conservation of momentum, energy, and angular momentum prevents the decay of the photon into two neutrinos. Conversely, the conservation of angular momentum is enough to ensure the stability of the neutrino with respect to decay into photons. Conservation of charge number guarantees the stability of the electron with respect to decay into neutrinos and photons. The stability of the proton is a more subtle phenomenon and requires for its explanation the conservation of a new quantum number—to wit, the baryon number. The assignment of baryon number 1 to the proton and of 0 to the photon, the leptons, and the mesons and the requirement of baryon number conservation then "explains" the stability of the proton. It is to be hoped that a deeper theoretical foundation will be found for the empirical principle of conservation

of baryon number, since, in a sense, it is this principle more than any other which is responsible for the stability of the universe as we know it.

Unstable Particles

We see that the principles of conservation of angular momentum, charge number, and baryon number (in addition to energy and momentum) are sufficient to ensure the stability of the photon, neutrino, electron, and proton. These principles are not sufficient to ensure the stability of the "heavy" lepton (that is, the muon), the mesons, and the baryons heavier than the proton. In actual fact, all of these particles turn out to be unstable. In Table 1 we have also tabulated the observed decay modes of the 12 unstable particles. We have not listed the decay modes of the antiparticles because the decay products must be the antiparticles of the decay products of the particle. Of the 12 unstable particles (and 11 unstable antiparticles) listed in Table 1, only two particles (the neutral pion and the neutral Σ-hyperon) and one antiparticle (the neutral anti-Σ-hyperon) emit photons in the dominant decay mode. It is to be noted that the lifetime of the neutral pion is less than 4×10^{-16} second and the lifetime of the neutral Σ-hyperon is less than 10^{-11} second (this is an upper limit obtained by a rather crude experiment). These are the shortest lifetimes in Table 1, and it is expected on theoretical grounds that both will lie in the range of 10^{-19} to 10^{-17} second. These extremely short lifetimes which are predicted for the two observed electromagnetic decays (as well as for the as-yet-unobserved neutral anti-Σ-hyperon) are still long on the nuclear time scale (by a factor of 10^4 or more, since the characteristic nuclear lifetime is 10^{-23} second—a nuclear dimension of 10^{-13} centimeter divided by the velocity of light) and justify the inclusion of these particles in the list of elementary particles. None of the other unstable particles shown in Table 1 emits photons in its dominant mode of decay.

The unstable particles whose dominant modes of decay do not involve emission of photons have lifetimes which range from 10^{-10} second to 10^3 seconds, a factor of 10^{13}. This enormous range in lifetime would appear to indicate a great diversity in the types of forces which are responsible for the corresponding decay processes. In actual fact, it seems possible to explain all of these decays by essentially a single type of interaction, which is called the universal weak force. The enormous range in lifetime is explained by a very sensitive dependence of the lifetime for weak decays on available energy (that is, the so-called Q value), ranging from a lifetime of 0.8×10^{-10} second and a Q value of 115 Mev for the Σ^+ lifetime to a lifetime of 10^3 seconds and a Q value of 0.75 Mev for the neutron. It is important to note that not all weak decay processes which are consistent with the conservation of angular momentum, charge number, baryon number, and lepton number are actually observed. For example, the decay process $\mu \rightarrow 3e$ has never been observed, although this process satisfies all the conservation laws satisfied by the well-known decay process $\mu \rightarrow e + \nu + \bar{\nu}$. Other examples could be given of weak decay processes which are not observed and which are as consistent with the absolute conservation laws and have as much available energy (so that the phase space factors are not unfavorable) as those which are observed. It appears necessary to introduce additional selection rules, other than those already cited, in order to understand this phenomenon.

Since the lifetimes of the three unstable particles and antiparticles whose dominant modes of decay involve the emission of photons are much shorter than the lifetimes of the 20 other unstable particles and antiparticles, whose dominant modes of decay do not involve the emission of photons, the question naturally arises as to why these electromagnetic modes of decay do not prevail for all unstable particles. Since an electromagnetic lifetime is almost certainly shorter than 4×10^{-16} second (the upper limit for the lifetime of the neutral pion), there is at least a factor of 10^6 in favor of an electromagnetic decay process over a nonelectromagnetic decay. The failure to take advantage of such a large factor can only be explained by some sort of selection rule which forbids the fast electromagnetic decays of the particles within each class (of leptons, mesons, and baryons). For example, within the baryon class, the potentially fast electromagnetic decays like $\Sigma^+ \rightarrow p + \gamma$ and $\Xi^- \rightarrow \Sigma^- + \gamma$ do not take place, whereas the fast electromagnetic decay process $\Sigma^0 \rightarrow \Lambda + \gamma$ does take place. Within the meson class, the potentially fast electromagnetic decay processes like $K^0 \rightarrow 2\gamma$ and $K^+ \rightarrow \pi^+ + 2\gamma$ do not take place, whereas the fast electromagnetic decay $\pi^0 \rightarrow 2\gamma$ does take place. Within the lepton class, the absence of the potentially fast electromagnetic decay process $\mu^- \rightarrow e^- + \gamma$ has to be understood.

Strangeness Quantum Number

We are therefore faced with the necessity of introducing a new quantum number in order to forbid the fast electromagnetic decay processes which could otherwise take place in accordance with the absolute conservation laws of angular momentum, charge number, baryon number, and lepton number. This new quantum number is the strangeness number S, and both the observed and unobserved electromagnetic decays can be understood if we make a suitable assignment of this quantum number S to each of the elementary particles and insist upon the conservation of S in all processes involving the electromagnetic force. In Table 1 we have listed the assignments of the strangeness number for each of the elementary particles. It is convenient to assign the value $S = 0$ to the lightest particle within each of the four classes—that is, to the photon, the "light" lepton, the pion, and the nucleon. Such an assignment, plus the requirement of S conservation, allows the fast electromagnetic decay process $\pi^0 \rightarrow 2\gamma$. It then seems appropriate to change this quantum number by one unit as one proceeds to the heavier members of each class of particles. In this fashion, one assigns a strangeness quantum number, $S = -1$, to the muon, although such an assignment is actually not needed in order to explain the absence of the fast electromagnetic decay $\mu^- \rightarrow e^- + \gamma$. An assignment of $S = +1$ to the kaon, $S = +-1$ to the Λ- and Σ-hyperons, and $S = -2$ to the Ξ-hyperon would then allow the fast electromagnetic decay $\Sigma^0 \rightarrow \Lambda + \gamma$ and forbid the fast electromagnetic decay of all the other particles within the meson and baryon classes. The strangeness number S, like Q, B, and L, is an "additive" quantum number, so that S for the antiparticle is the negative of S for the particle.

It is interesting to note that while the weak decay processes are consistent with the absolute conservation laws of angular momentum, charge number,

baryon number, and lepton number, they are not consistent with the conservation of strangeness number, as the fast electromagnetic decays are. Examination of the decay modes in Table 1 shows that in the weak decays, the strangeness number may remain unaltered (compare the neutron decay) or may change by one unit (compare the Λ decay or the K decay); it appears that the selection rule for the weak decays is $\Delta S = 0 \pm 1$. In the case of the Ξ-hyperon, where a weak decay with $\Delta S = 2$ could take place (for example, $\Xi^- \rightarrow n + \pi^-$), there is no evidence for such a process despite the fact that more phase space is available than for the decay $\Xi^- \rightarrow \Lambda + \pi^-$ which does take place. It appears that a change of ΔS by as much as two units is not allowed even for the processes governed by the weak force.

Of course, it is not remarkable that the conservation of the strangeness number S explains the observed facts concerning the fast electromagnetic decays of the elementary particles since, in a sense, we have simply introduced this new quantum number in order to account for these facts. What is remarkable is that the same conservation law of strangeness number holds for the strong or nuclear-type forces between mesons and baryons and has been confirmed in a wide variety of ways for physical processes governed by these strong forces. First of all, we have an explanation of the reason why decays such as $K^+ \rightarrow \pi^+ + \pi^0$, $\Lambda \rightarrow p + \pi^-$, $\Sigma^+ \rightarrow p + \pi^0$, and so on, do not take place extremely rapidly (with lifetimes of the order of 10^{-23} second—the nuclear lifetime—by virtue of the strong forces); they all violate the conservation of the strangeness number. The much longer lifetimes which are observed correspond to decays which occur through the weak forces and are permitted even though the strangeness number is not conserved. It is to be noted that all of the above reactions are consistent with the conservation laws of charge, baryon, and lepton numbers (in addition to energy, momentum, and so on).

A second important consequence of strangeness conservation for the strong forces is that the production of particles with $S \neq 0$ (called strange particles) must take place at least in pairs if the initial state only involves ordinary particles (that is, particles with $S = 0$, such as pions and nucleons). Thus, in nucleon-nucleon and in pion-nucleon collisions, the production of Λ- and Σ-hyperons requires the simultaneous production of the K^0 and K^+ mesons. The production of Ξ-hyperons requires the simultaneous production of two kaons with positive strangeness number. In other words, there is associated production of strange particles. Moreover, strangeness conservation implies that the production of the K^- or $\overline{K^0}$ meson can only take place if there is sufficient energy to produce simultaneously the K^+ or K^0 meson; this provides an immediate explanation of the much lower energy threshold observed for production of K^+ mesons than for K^- mesons, a difference that exists despite the fact that the masses of the positive and negative kaons are identical. It is found that the imposition of the requirement of conservation of the strangeness number is consistent with all physical processes which occur by virtue of the strong and electromagnetic forces but is not consistent with all the weak processes.

Three Types of Forces

In commenting upon the properties of the elementary particles I have referred to three types of forces which are responsible for the various types of processes in which the elementary particles are involved. The three types of forces (which at this stage, do not seem to bear any relationship to each other) are, to repeat, the strong or nuclear force, the electromagnetic force, and the weak force. Of the four classes of elementary particles—photon, lepton, meson, and baryon—the photon is the carrier, so to speak, of the electromagnetic force and does not exert a weak or strong force on any of the other particles. The charged members of the lepton class of particles experience electromagnetic forces due to the other particles (the neutrino does not, because it possesses no charge and no magnetic moment), whereas all the members of the lepton class interact weakly with each other and with the members of the meson and baryon classes. As regards the meson and baryon classes, we can say that, in general, the members of these two classes interact strongly with each other, interact electromagnetically with all the elementary particles, and interact weakly with each other and with the members of the lepton class.

As I have stated, the three forces under consideration are, in order of decreasing strength, strong or nuclear force, electromagnetic force, and weak force. It is useful to give a quantitative estimate of a dimensionless constant which is characteristic of each of the forces. The most familiar of the forces, the electromagnetic, is the one of intermediate strength, where the dimensionless constant which crops up whenever any electromagnetic process is calculated is the fine-structure constant

$$\alpha = \frac{e^2}{4\pi\hbar c} = \frac{1}{137}$$

where e is the elementary charge, $\hbar$ is Planck's constant over 2π, and c is the velocity of light. It is important to note that the charge e, which enters into the fine-structure constant, is a measure of the strength of the interaction of a charged particle with the electromagnetic field. The analog of the fine-structure constant in the case of the strong or nuclear force is a dimensionless constant $g^2/4\pi\hbar c \cong 15$, where g is the pion-nucleon coupling constant. The pion-nucleon coupling constant has been chosen because its value is much better known from pion scattering experiments (on nucleons) and from the nuclear force between two nucleons (which is attributed chiefly to the pion field).

For both the electromagnetic and strong forces, a dimensionless constant emerges in a natural way by considering e or g, which measures the strength of the corresponding force. In the case of the weak force, the characteristic coupling constant has a different dimension, and it is necessary to perform some manipulations in order to obtain a dimensionless constant characteristic of the weak force. The coupling constant for β decay, G, which is the most extensively studied of the weak forces, has the value 1.4×10^{-49} erg $\times$ cubic centimeter. A dimensionless constant characteristic of the weak force is then obtained by taking $G^2 \times (\hbar c)^{-3} \times (\hbar/\mu c)^{-4} \simeq 5 \times 10^{-14}$ (μ is the pion mass). In order to obtain a dimensionless constant, we have used the pion Compton wavelength ($\hbar/\mu c$); however, it is clear that the result which would be obtained by utilizing the Compton wavelength of some other particle would not alter the essential conclusion, which is that the weak force is extremely weak (by many powers of 10) as compared to the electromagnetic and strong forces.

There is one further force which is extremely important for the macro-

scopic world, to which no reference has thus far been made—namely, the gravitational force. However, the dimensionless constant characteristic of the gravitational force, which is obtained by using the (large) baryon mass, is $KM^2/\hbar c \simeq 2 \times 10^{-39}$ [K ($= 6.7 \times 10^{-8}$ dyne cm^2/gm^2) is the gravitational constant]. This extraordinarily small number justifies the complete neglect of the gravitational force in discussing the elementary particles.

I have already indicated that certain absolute conservation laws (of energy, momentum, angular momentum, charge number, baryon number, and lepton number) govern all three types of force which are of importance for the elementary particles. I have also referred to at least one conservation law which holds for some of the forces but not all—that is, the conservation of strangeness number is supposed to be valid for the electromagnetic and strong forces but not for the weak forces. There is at least one additional absolute conservation law (time reversal invariance) which holds for all three types of force, and there are at least three approximate conservation laws (parity conservation, charge conjugation invariance, and isotopic spin conservation) which hold for at least one of the forces but not for all. It appears that the strong forces are governed by the largest number of conservation laws (and therefore are subject to the largest number of invariance principles), with the electromagnetic interactions governed by fewer and the weak interactions subject to the least number of conservation laws. The correlation between the strength of the interaction and the number of conservation laws is certainly one of the important problems in the field of elementary particle physics.

Conservation of Parity, Charge Conjugation, Time Reversal

In conclusion, I shall comment briefly on the conservation laws of parity, charge conjugation, and time reversal. These conservation laws were accepted as a priori principles for many years. Then in 1956 came the shocking demonstration of parity breakdown in beta-decay processes, and since then all three conservation laws have been reinvestigated experimentally under all types of conditions. We can summarize the experimental situation at the present time as follows. In the domain of strong forces, an experimental search for parity nonconservation has been made in connection with ordinary nuclear forces, the πN (pion-nucleon) force, and the ΛKN force. No evidence has been found for parity nonconservation. There is similarly no experimental evidence for parity nonconservation in electromagnetic transition in atoms and nuclei. We may conclude that, just as for the strong forces, there is no experimental evidence for parity nonconservation in electromagnetic forces.

The situation is spectacularly different for the weak forces. There are literally dozens of experiments where parity nonconservation has been demonstrated in an unambiguous fashion. Indeed, one of the most striking aspects of all of these experiments is that the parity breakdown is usually complete. Parity nonconservation effects have been found in all types of weak processes: in connection with the leptonic decays of the nucleon, muon, pion, and kaon and the nonleptonic decay modes of the hyperons. We are thus led to the over-all conclusion that there is absolutely no experimental evidence for the nonconservation of parity for the strong and electromagnetic forces but that there is overwhelming evidence for the maximum nonconservation of parity for all the weak forces, whether they involve leptons, mesons, or baryons, strange or non-strange.

Next, consider the experimental evidence for the charge conjugation invariance of the strong, electromagnetic, and weak forces among the elementary particles. In order to decide whether a force is invariant under charge conjugation, we must compare a given physical process governed by this force with the charge conjugation transform of this physical process—namely, the physical process obtained by replacing every particle by its antiparticle. If the physical process and its charge conjugation transform governed by a given force are not identical (with respect to their cross sections, properties of the decay products, and so on), we can conclude that the force is not invariant under charge conjugation. It turns out that the experimental situation with regard to charge conjugation invariance is similar to that for parity conservation. Both the strong and electromagnetic forces are invariant under charge conjugation, whereas there is unequivocal experimental evidence for the breakdown of charge conjugation invariance in physical processes governed by the weak force. In the case of the weak force, direct experimental evidence for the failure of charge conjugation invariance comes from the observation that the helicities (that is, left- or right-handedness) of the electron and positron are opposite from the μ^- and μ^+ decays, respectively; that the helicities of the μ^- and μ^+ are opposite from the π^- and π^+ decays, respectively; and that the helicities of the neutrino and antineutrino are opposite in all decays in which they are involved. If these weak decays were invariant under charge conjugation, the particles and antiparticles would have to come out with the *same* zero helicity. Since the particles come out with *opposite* finite helicities, the breakdown of charge conjugation invariance is proved for the weak force.

A serious effort has also been made to ascertain whether time reversal invariance breaks down for any of the three forces. All the available evidence is in favor of the time reversal invariance of all physical processes—those governed by the weak force as well as those governed by the strong and electromagnetic forces.

Now there is a famous (*"CPT"*) theorem which says that if physical processes obey Einstein's laws of special relativity, then they will be invariant under the product of the operations of charge conjugation (C), parity (P), and time reversal (T). We therefore reach the interesting conclusion that since all the forces among the elementary particles (strong, electromagnetic, and weak) appear to be invariant under the time reversal operation, they must be invariant under the product of the charge conjugation and parity operations (that is, we have CP invariance). This is a trivial statement for the strong and electromagnetic forces, since they are separately invariant under C and P. However, the weak forces are separately *noninvariant* under C and P, but nevertheless it turns out (as a result of the CPT theorem and their invariance under T) that they are invariant under the product of the two operations C and P. An understanding of the fact that all forces among the elementary particles are CP-invariant, with the weak force separately noninvariant under C and P, is another intriguing problem of elementary particle physics.

Note

1. The Russians have recently produced an anti-Σ-hyperon with their 10-Bev accelerator at Dubna.
2. These experiments are discussed in G. Feinberg and M. Goldhaber, *Proc. Natl. Acad. Sci. U.S.* **45** (1959).

Hybrid Corn and the Economics of Innovation

Geographic differences in the use of hybrid corn are explained by differences in the profitability of that use.

Zvi Griliches

The idea that a cross between plants that are genetically unlike can produce a plant of greater vigor and yield than either of the parental lines dates back to Darwin and earlier. Serious research on hybrid corn, however, did not begin until the first years of this century, and the first application of research results on a substantial commercial scale was not begun until the early 1930's. During the last 25 years, the change from open pollination to hybrid seeds has spread rapidly through the Corn Belt, and from the Corn Belt to the rest of the nation. The pattern of diffusion of hybrid corn, however, has been characterized by marked geographic differences. As shown in Fig. 1, some regions began to use hybrid corn much earlier than others, and some regions, once the shift began, made the transition much more rapidly than others. For example, Iowa farmers began planting hybrid corn earlier than did Alabama farmers, and Iowa farmers increased their acreage in hybrid corn from 10 to 90 percent more rapidly than did Alabama farmers.

Although the explanation of area differences in the pattern of diffusion of hybrid corn constitutes the main contribution of the study reported here (*1*), it is worth drawing attention first to the striking similarity in the general pattern of diffusion of hybrid seed in the various areas. Almost everywhere the development followed an S-shaped growth curve. As illustrated in Fig. 1, the rate of change is slow at first, accelerating until it reaches its peak, at approximately the mid-point of development, and then slowing down again as the development approaches its final level (*2*). Interestingly enough, this pattern of development also applies to increases in the use of farm equipment—combines, corn pickers, pickup balers, and field forage harvesters, as illustrated in Fig. 2. Similar patterns occur in the use of new drugs by doctors and in the diffusion of other new items or ideas (*3*). Thus, the data on hybrid corn and other technical changes in U.S. agriculture support the general finding that the pattern of technical change is S-shaped.

Although the finding that technical change follows this pattern is not very surprising or new (*4*), it is very useful. It allows us to summarize large bodies of data on the basis of three major characteristics (parameters) of a diffusion pattern: the date of beginning (origin), relative speed (slope), and final level (ceiling). The interesting question then is, given this general S-shape, what determines the differences among areas in the origin, slope, and ceiling? Why were some areas ahead of others in first using hybrid corn? Why did hybrid corn spread faster in some areas than in others? Why did some areas reach higher levels of equilibrium than others?

Date of Availability

Although the *idea* of breeding hybrid corn as we know it today goes back at least to 1918, to D. F. Jones and the double cross, the dates at which superior hybrids actually became available in different areas varied widely. Hybrid corn was not a once-and-for-all innovation that could be adopted everywhere. Rather, it was an invention of a new method of innovating, a method of developing superior strains of corn for specific localities (*5*). The actual process of developing superior hybrids had to be carried out separately for each locality. It is important to remember this fact before one blames, for example, the southern farmers for being slow to plant hybrid corn. Although superior hybrids became available in the Corn Belt in the early 1930's, it was only in the middle of the 1940's that good hybrids began to appear in the South. Thus, the date for a given area on which commercial quantities of superior hybrid seed were first produced is one of the major determinants of the development in that area.

We can take the date on which an area began planting 10 percent of its corn acreage to hybrid corn as the date on which superior hybrids became available to farmers in commercial quantities. As shown in Fig. 3, different areas in the United States reached the 10-percent level on different dates. For example, this level was reached in 1936 in some parts of Iowa and in northern Illinois but was not reached until after 1948 in some parts of Alabama and Georgia. The usefulness of the 10-percent level as a measure of the commercial availability of hybrid corn seed is indicated by the very close correspondence between this and an alternative measure. From records of state yield tests and from other publications it is possible to determine in what year hybrids first outyielded open-pollinated varieties by a substantial margin in a given locality. The "10-percent" definition used in this study has a .93 Spearman rank correlation coefficient with the "technical" definition.

Area differences in date of first planting of hybrid corn can be explained in terms of differences in date of availability of hybrid corn seed. Area differences in date of availability, in turn, can be explained, in part, in terms of some simple economic factors. Innovators among the seed producers first entered those areas where the expected profits from the commercial production of hybrid corn seed were largest. They entered the "good" areas ahead of the "poor" ones. It is no accident that, though the major innovation occurred in Connecticut, commercial development began in the heart of the Corn Belt where the potential market—farmers who buy and plant corn seed—was largest. The profits that seed producers can expect to make in a given region depend upon the size of the market for corn seed in that region and the cost of entry in that region.

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Market Density and Cost of Entry

The close correlation for an area between the date of availability of hybrid corn seed and the market for corn seed may be seen by comparing Fig. 3 with two reasonable measures of the market. The first measure is the density of corn acreage in 1949, shown in Fig. 4; the second measure is the density of corn pickers in use on farms, shown in Fig. 5 (the second measure is the better index of the "goodness" of a corn area and provides the best simple outline of the Corn Belt). This correlation is also demonstrated by plotting the date of entry of hybrid-seed producers into an area, as measured by the date on which farmers devoted 10 percent of their corn acreage to hybrid corn, against the average market density of the area. As shown in Fig. 6 the lower the market density, the later the date of entry into a given area. The rank correlation is high (.7), and even higher (.9) if the Southeast is excluded from the computations. The Southeast is a special case. It was entered later because of the relative lateness of the research contributions by the region's experiment stations and the obstacles put in the way of private seed companies in that area. Moreover, when one gets down to a certain low level it does not really pay to discriminate between areas on the basis of their relative market densities, and other variables become more significant.

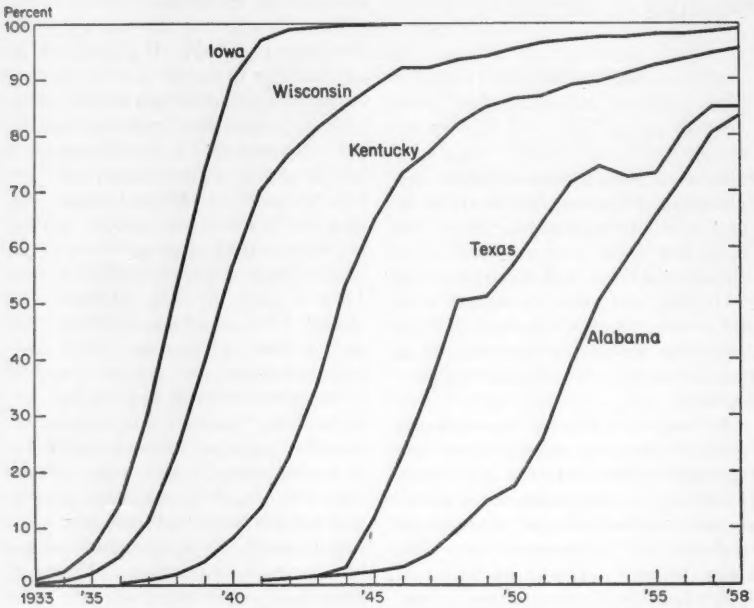


Fig. 1. Percentage of all corn acreage planted to hybrid seed. [*Agricultural Statistics*]

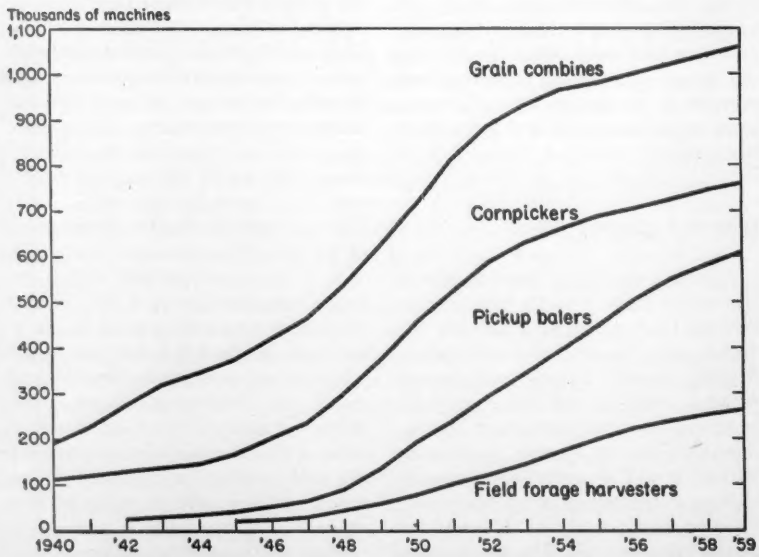


Fig. 2. Machines in use on farms in the United States, 1940–59. Note the resemblance to Fig. 1. [*U.S. Dept. Agr. Statist. Bull. No. 233* (1959)]

Deviations in the correlation between the spread of hybrid corn and the distribution of the market can be explained by the cost of entry factor. Cost of entry depends, among other things, on how different the area is from those already entered, and on whether experiment stations have already developed inbred lines and whole hybrids adaptable to the area. Study of Figs. 3, 4, and 5 shows first that the spread was much faster latitudinally than it was longitudinally. The reason, in part, is that an important factor determining the range of adaptability of a particular hybrid is the length of the growing season. To a large extent this is a function of latitude, and as one moves east or west the chances that the same hybrid will be adaptable to new areas are much higher than they are if one moves north or south. Nevertheless, the movement north seems to have been faster than the movement south. This is partly because of the larger markets in the north but is also a reflection of the special contributions of the Minnesota and Wisconsin agricultural experiment stations. They entered hybrid corn research very early in the game and contributed a great deal more than one would have expected from them just on the basis of the relative importance of corn in their states. Similarly, the contributions of Texas, Louisiana, and Florida stations came earlier and were relatively larger than those of the other stations in the South, which produced little of importance till the middle 1940's. This would explain to some extent why hybrid corn moved into the Southwest before it did into the Southeast. Moreover, quite a few of the Corn Belt inbreds and hybrids proved adaptable in the Southwest. It was more like the Corn Belt than was the Southeast, and it did not suffer to the same extent

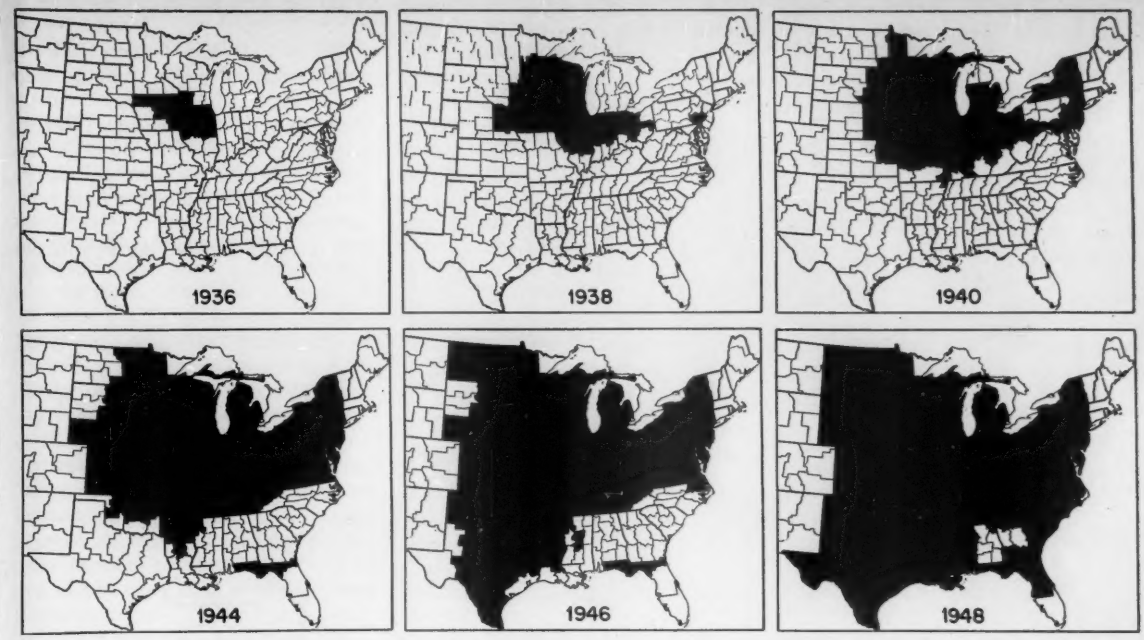


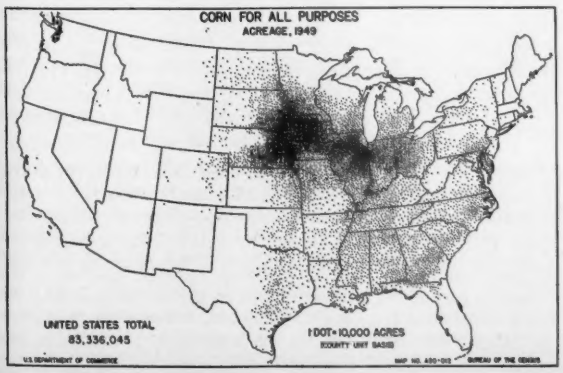
Fig. 3. The spread of hybrid corn: Areas that planted 10 or more percent of their corn acreage to hybrid seed in specified years.

from insect and disease problems that corn-breeders in the Southeast had to deal with (*6*).

Since cost factors in areas that are close together are likely to be similar, we may assume that entry into the neighborhood of an area makes entry into the area itself more likely, and thus we may use the earliest date of entry into any of the immediately neighboring areas as a proxy variable for the cost of entry into the given area. This variable and the measure of market density, taken together, explain to a large extent the variability in the dates on which hybrid corn was introduced in different parts of the country and support the statement that the innovators were influenced by considerations of profit, entering those areas first where the expected profits from innovation were highest.

Rates of Acceptance

The rate at which farmers in a region accepted hybrids, once hybrid corn became available, also varied from area to area. As shown in Fig. 7, this rate was highest in Iowa and the surrounding area and lowest in some of the areas of the Southeast and the Mississippi Delta states. The differences in the rates of acceptance are largely demand phenomena, not a result of different supply conditions. After the first few years, in most of the places and most of the time, the supply of seed was not the limiting factor. The rate of acceptance is taken to be the relative speed of the diffusion process—that is, it is the slope coefficient of the S-shaped curve (*7*). The measure is such that a value of 1.00 means that it takes 4 years for the acreage devoted to hybrid corn to rise from 12 to 88 percent, while a value of 0.5 implies that it would take 8 years, or



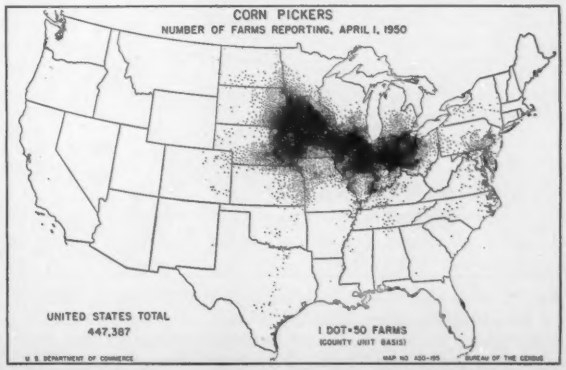


Fig. 4 (left). The market for hybrid seed. Corn acreage in 1949. Fig. 5 (right). Corn pickers on farms, 1950. This is a better index than corn acreage of the "goodness" of an area with respect to corn growing and provides the best single outline of the Corn Belt.

twice as long, for this same rise to occur.

The rate at which farmers accept a new technique depends, among other things, on the magnitude of the profit to be realized from the change-over. This hypothesis is based, first, on the general observation that the larger the stimulus the faster the reaction to it, and, second, on the fact that in an uncertain environment it takes a shorter time to find out that there is a difference, if that difference is large (*8*). Farmers doubted that this new hybrid corn was any good, and it took them some time to become convinced of its superiority. Individual farmers followed a development pattern of their own in shifting from open-pollinated to hybrid seed in planting their corn acreage (see Fig. 8). Almost no farmer planted 100 percent of his corn acreage to hybrid seed the first time he tried it (*9*).

Yield per Acre and Acres per Farm

The rate at which farmers shifted to hybrid corn depends, among other things, upon the profitability of such a shift. This in turn depends upon the absolute superiority of hybrids in corn yield in bushels per acre, and on the average number of acres per farm planted to corn.

It is widely accepted that hybrids out-

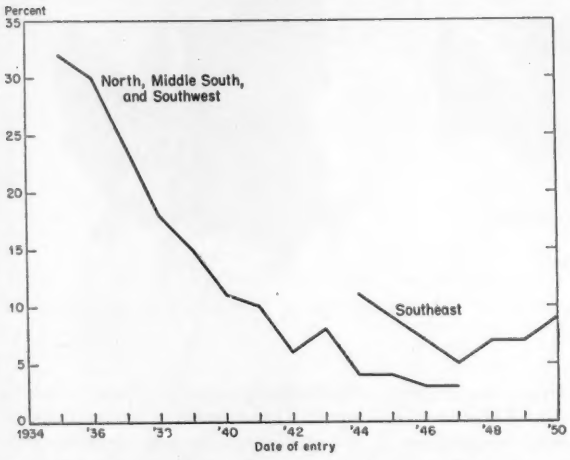


Fig. 6. Average market density by date of entry: Corn acres as a percentage of land in farms for crop-reporting districts reaching 10-percent use of hybrid seed corn in a specified year.

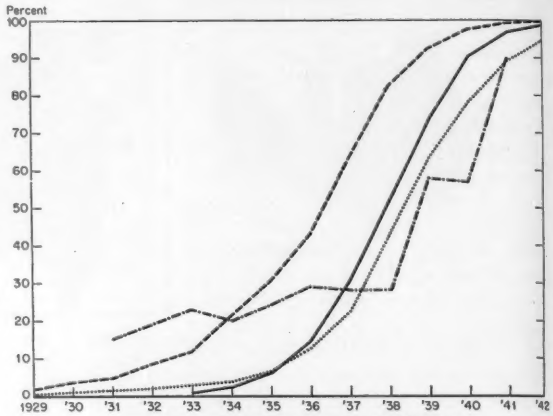


Fig. 8. The acceptance of hybrid corn in Iowa. Each farmer planted only a small fraction of his corn acreage to hybrid seed on his first trial. Only very late in the spread process did the "first timers" become bolder. [B. Ryan, *Rural Sociol.* **13**, 273 (1948); *Agricultural Statistics.*]

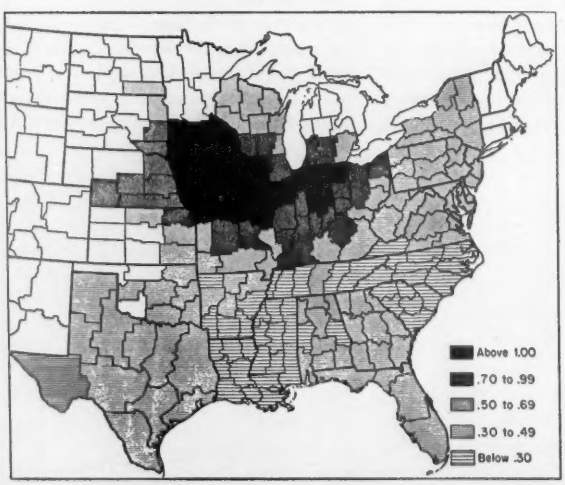


Fig. 7. Estimated rate of acceptance of hybrid corn by farmers in different parts of the country. A value of 1.00 means that it took 4 years for an area to increase its percentage of corn acres planted to hybrid seed from 12 to 88 percent. A value of 0.5 implies that it took twice as long, 8 years, to accomplish the same change.

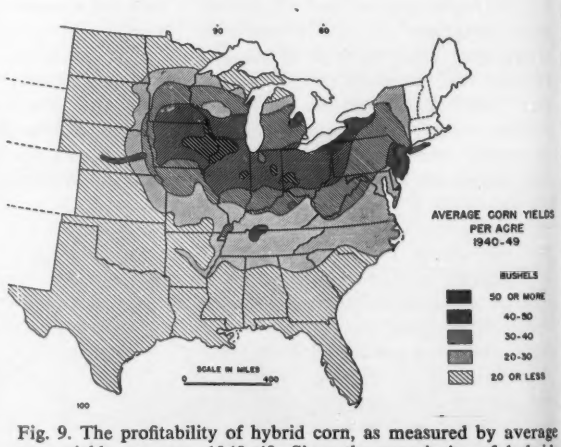


Fig. 9. The profitability of hybrid corn, as measured by average corn yields per acre, 1940–49. Since the superiority of hybrids was a constant percentage at different yield levels, differences in the *absolute* superiority of hybrids between areas are indicated by differences in the long-run levels of corn yields in the various areas. The period 1940–49 is neither strictly a pre- nor a post-hybrid period, but the differences in the levels of hybrid seed use in different areas during this period are unlikely to affect the relative ranking of the various regions with respect to their long-run corn-yield potentials. [From A. Grotewold, *Regional Changes in Corn Production in the U.S. from 1909 to 1949* (Chicago, 1955)]

yielded open-pollinated varieties by approximately 15 to 20 percent and that this *percentage* superiority did not vary much between different areas (*10*). Since similar percentage increases in yield imply different absolute increases in bushels per acre in areas where the previous yields were different, a good measure of the absolute superiority of hybrids over open-pollinated varities is given by the long-run level of corn yields in various areas. The distribution of corn yields in the United States, as shown in Fig. 9, shows strikingly close correlation with the distribution of rates of acceptance of hybrid corn, as shown in Fig. 7; The higher the yield, the higher the rate of acceptance.

Where the rate of acceptance fails to correlate with the yield per acre, the failure can be explained by taking into account the difference in the average number of acres of corn per farm (corn acreage per farm increasing as one moves from East to West), since what is important is not only the profitability per acre but also the profitability per farm. A large fraction of the variability between areas in the rate of acceptance of hybrid corn by farmers can be explained with the help of these two "profitability" variables (*11*).

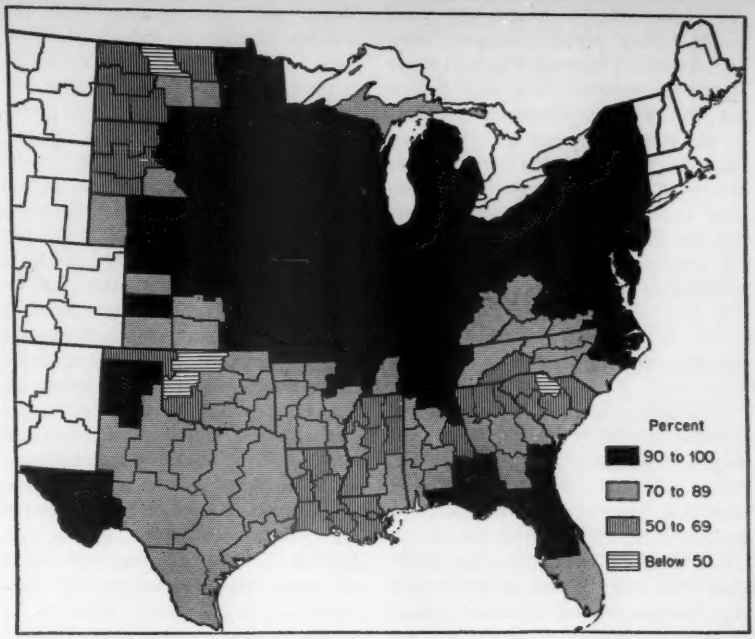


Fig. 10. Hybrid corn today (1959). Approximate percentage of total corn acreage planted to hybrid seed. State Agricultural Statisticians' release and unpublished Agricultural Marketing Service data.

Equilibrium Level

In an analysis of the use of hybrid corn in this country, one must consider, finally, differences in the equilibrium level reached—that is, differences in the fraction of the acreage which is ultimately devoted to hybrid seed. As shown in Fig. 10, different levels were found in different areas of the country. By 1959, close to 100 percent of the corn acreage in most of the Corn Belt and in its northern and eastern fringes was planted to hybrid seed. Substantially lower percentages were found only in the western fringes of the Corn Belt and in the deep South. In the South, the level is still changing, moving towards an equilibrium level of approximately 70 to 80 percent of the corn acreage planted to hybrid seed. The western parts of Nebraska, South Dakota, and Kansas have already reached their equilibrium level of approximately 30 to 60 percent. These are areas of very low and very variable yields, where the use of hybrid seed is unprofitable except on the better land or on land under irrigation.

Differences in the equilibrium level are explained by differences in the *average* profit to be realized from the shift to hybrid seed. In an area of high average profit no farmer faces a loss from the shift. In areas of low average profit a substantial proportion of the farmers face the possibility of having no return or even of sustaining a loss on their investment. The ceiling, or the fraction of the corn acreage that will ultimately be planted to hybrid seed, is not unique or constant. It will change with the introduction of better hybrids, with improvements in the market for corn, and with large changes in corn acreage. Nevertheless, for almost all of the areas except the very marginal ones, a constant ceiling fits the data well. Variation in these ceilings across the country can be explained in good part by the same two measures of profitability: the average absolute superiority of hybrids and the average number of acres per farm planted to corn.

Relation to Studies by Sociologists

It may prove useful to relate the results of this study to earlier work by sociologists in this area (*12*). In previous analyses of similar data it was mainly individual behavior that was investigated—that is, Who are the first and who are the last to adopt hybrid corn? —and an attempt was made to explain such behavior on the basis of differences in personality, education, economic status, and social environment. An attempt to use some of these variables (for example, level-of-living indexes) in explaining differences between states in the rate of acceptance of hybrid corn proved unsuccessful in this study.

It is my belief that in the long run, and when the country is taken as a whole, many of these variables either do not vary enough to be significant or tend to cancel themselves out, leaving the economic variables as the major determinants of the patterns of technological changes. This does not imply that the "sociological" variables are not important if one wants to know which *individual* will be first or last to adopt a particular technique, only that these factors do not vary much from area to area. Moreover, the distinction between "economic" and "sociological" variables is partly semantic, and a very difficult one to make in practice. Some of the variables used in this study—for example, yield of corn and corn acres per farm—are closely correlated with such variables as education, level-of-living, and socioeconomic status. It is very difficult to discriminate between the validity of the assertion that hybrids were accepted slowly because it was a "poor corn area" and the assertion that the

slow acceptance was due to "poor people." Poor people and poor corn are very closely correlated in the United States. Nevertheless, one may find a few areas where this is not so. Obviously, the slow acceptance of hybrids on the western fringes of the Corn Belt—in western Kansas, Nebraska, South Dakota, and North Dakota—does not reflect low economic status of the people but is the result of "economic factors" which make this a poor corn area.

Summary and an Implication

This study has increased our understanding of a body of data. What were originally puzzling and seemingly peculiar patterns in the data have been explained. The use of hybrid seed in an area depends, in part, upon the date at which superior hybrids become available. This date, in turn, depends upon the activities of seed producers guided by their expectations of profits, and upon the contributions of the various experimental stations. Thus, the South was late in getting hybrids because the market for seed was substantially poorer there than in other areas and because southern experiment stations produced few hybrids of importance until the middle 1940's. The use of hybrid seed in an area also depends upon the rate at which hybrids are accepted by farmers. This rate, in turn, depends upon the profit farmers expect to realize from the shift to hybrids. Thus, farmers in the Corn Belt accepted hybrids at a faster rate than farmers in the South because the absolute magnitude of profit was higher in the Corn Belt than in the South. Similarly, the fraction of acreage ultimately planted to hybrid seed depends upon expectations of profits to be realized from the change and the distribution of these expectations around their mean.

When uncertainty and the fact that the spread of knowledge is not instantaneous are taken into account, it appears that American seed producers and American farmers have behaved, on the whole, in a fashion consistent with the idea of profit maximization. Where the evidence appears to indicate the contrary, I predict that a closer examination of the relevant economic variables will show that the change was not as profitable as it appeared to be (*13*).

This study of hybrid corn has at least one interesting implication. Hybrid corn was an innovation which was more profitable in the "good" areas than in the "poor" areas. This, probably, is also a characteristic of many other innovations. Obviously, tractors contribute more on large than on small farms, and so forth. Hence, there may a tendency for technological change to accentuate regional disparities in levels of income and rates of growth. Moreover, this tendency is reinforced by the economics of the innovation process, which results in the new techniques being supplied to the "good" areas before they are supplied to the "poorer" areas, and also in the more rapid acceptance of these techniques in the "good" areas. A lag of this sort can by itself cause long-run regional differences in levels of income. The kinds of inventions we get, and the process by which they are distributed, may lead to aggravation of the already serious problem of regional differentials in levels of income and growth.

References and Notes

1. A more detailed and technical account of this study can be found in Z. Griliches, "Hybrid corn: An exploration in the economics of technological change," *Econometrica* (Oct. 1957). See also my "Research costs and social returns: Hybrid corn and related innovations," *Political Econ.* (Oct. 1958). This study was supported by the Social Science Research Council and the National Science Foundation.
2. In my article in *Econometrica* I show that the data fit the logistic growth curve very well. Unpublished data by small subdivisions —county and crop-reporting districts—give essentially the same picture, though the development is somewhat more irregular in the marginal corn areas.
3. See J. Coleman, E. Katz, H. Menzel, "The diffusion of an innovation among physicians," *Sociometry* (Dec. 1957).
4. See, for example, A. Lotka, *Elements of Physical Biology* (1925) and S. Kuznets, *Secular Movements in Production and Prices* (1930).
5. "Hybrid corn is the product of a controlled, systematic crossing of specially selected parental strains called 'inbred lines.' These inbred lines are developed by inbreeding, or self-pollinating, for a period of four or more years. Accompanying inbreeding is a rigid selection for the elimination of those inbreds carrying poor heredity, and which, for one reason or another, fail to meet the established standards" [N. P. Neal and A. M. Strommer, "Wisconsin Corn Hybrids," *Wisconsin Univ. Agr. Expt. Sta. Bull. No. 476* (Feb. 1948), p. 4]. "[The inbred lines] are of little value in themselves for they are inferior to open-pollinated varieties in vigor and yield. When two unrelated inbred lines are crossed, however, the vigor is restored. *Some* of these hybrids prove to be markedly superior to the original varieties. The development of hybrid corn, therefore, is a complicated process of continued self-pollination accompanied by selection of the most vigorous and otherwise desirable plants. These superior lines are then used in making hybrids" [R. W. Jugenheimer, "Hybrid Corn in Kansas," *Kansas Agr. Expt. Sta. Circ. No. 196* (Feb. 1939), pp. 3–4].
6. One can approximate a measure of the difference between the Corn Belt and some other areas with respect to the kind of hybrids they take by computing from lists of recommended hybrids in various areas and their pedigrees the percentage of all inbred lines accounted for by Corn Belt inbred lines. Such an index of "Cornbeltlines" had a correlation of .8 with the "date of origin" on the state level and of .7 with that on the crop-reporting-district level.
7. More exactly, the measure is the slope coefficient of the logistic growth curve as adjusted for ceiling differences. For an explanation of this measure see Z. Griliches, *Econometrica* (Oct. 1957).
8. This is analogous to sequential sampling. The "average sample number"—that is, the expected length of the experiment—will depend, among other things, inversely on the difference between the means of the two populations being sampled and directly on their variance.
9. That this is rational behavior in the face of uncertainty is intuitively obvious but difficult to prove. See H. A. Simon, *Models of Man* (New York, 1957).
10. Of course, hybrids differed from open-pollinated varieties not only in yield but also in improved stand, in uniformity, and in resistance to disease. However, there are no good quantitative measures of improvement in factors other than yield, and besides, most of the other improvements were correlated with the increases in yield.
11. Similar results were obtained from unpublished Agricultural Marketing Service data on the actual difference in the yield of hybrid and of open-pollinated varieties as a measure of the superiority of hybrids. Both the fit and the coefficients were very similar.
12. See, for example, B. Ryan and N. Gross, "Acceptance and Diffusion of Hybrid Corn Seed in Two Iowa Communities," *Iowa Agri. Expt. Sta. Research Bull. No. 372* (Jan. 1950), and "How Farm People Accept New Ideas," *Iowa State Coll. Spec. Rept. No. 15* (Nov. 1955).
13. That these findings are not restricted to hybrid corn has been confirmed by a recent study of the spread of a series of industrial innovations (diesel locomotives, continuous mining machines, and so on) within particular industries. It was found there that (i) the logistic growth curve summarized the data well, and (ii) most of the variability in the rate of acceptance of different innovations can be explained on the basis of the relative profit to be realized from the innovation and the size of the required initial investment. See E. Mansfield, "Technological change and the rate of imitation," a paper that was read at the 1959 winter meetings of the Econometric Society in Washington, D.C.

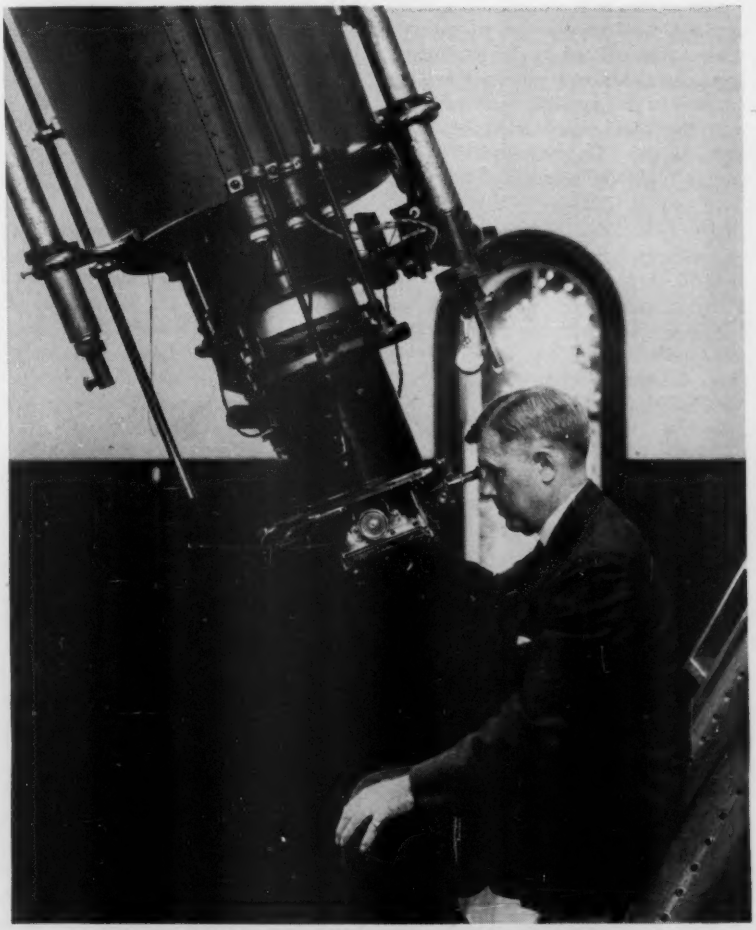
Samuel Alfred Mitchell, Solar and Stellar Astronomer

Samuel Alfred Mitchell was born in Kingston, Ontario, on 29 April 1874 and died in Bloomington, Indiana, on 22 February 1960. He received the master of arts degree at Queens College in 1894 and the degree of doctor of philosophy in astronomy at Johns Hopkins in 1898, then spent one year at the Yerkes Observatory as research assistant in astronomy. From 1899 to 1913 he taught at Columbia University. In 1913 he succeeded Ormond Stone as professor of astronomy and director of the Leander McCormick Observatory of the University of Virginia. He became director emeritus in 1945.

Mitchell's scientific activities broadly covered three fields: stellar parallax and related problems, variable stars, and solar eclipses. During his 32 years of directorship, the McCormick Observatory developed into an important center of astronomical research. One of the most urgent astronomical problems at the beginning of the 20th century was that of determining stellar distances. In 1907, and again between 1909 and 1913, Mitchell participated in the pioneering work of Frank Schlesinger on the determination of trigonometric stellar parallaxes from photographs taken with the long-focus refractor of the Yerkes Observatory. Mitchell attacked this problem with the 26-inch McCormick refractor, following Schlesinger's methods. With the assistance of C. P. Olivier (now director emeritus of the Flower and Cook Observatory), H. L. Alden (the present director of the McCormick Observatory), and numerous others, Mitchell made the McCormick Observatory into one of the leading observatories contributing to photographic parallax determinations. A first volume (McCormick Observatory *Publications*, vol. 3), containing 260 parallaxes, was published in 1920, to be followed by volumes 4, 8, and 14; the work is being continued.

In 1922, the Dutch astronomer Kapteyn pointed out the potential importance of parallax plates for the determination of accurate proper motions, particularly in regions centered on bright stars, and the importance of these findings for the determination of proper motions of faint field stars, referred to the system of the bright stars. Mitchell and Alden recognized the significance of this new field, which now became an additional program of the McCormick Observatory. Mitchell invited Peter van de Kamp from the Kapteyn Astronomical Laboratory in Groningen to participate in this work, which was destined to continue for several decades and led to important statistical knowledge of the motions of faint stars, such as the determination of preferential motion, solar motion, galactic rotation, and precessional constants. This proper-motion work reached its highest activity during the 1930's and 1940's, as carried out by A. N. Vyssotsky and E. T. R. Williams. The results of the work are found primarily in volumes 4, 7, and 10 of the McCormick *Publications*.

Another development of the original parallax work was the determination of mass ratios of binary stars which had been photographed over several decades. A further by-product of the initial parallax program was the occasional discovery of perturbations of single stars, which pointed to the existence of unseen companions. The classi-



Samuel Alfred Mitchell [Virginia State Chamber of Commerce]

cal photographic example is the faint red dwarf star Ross 614, which has a perturbed motion first discovered, in 1935, by D. Reuyl at the McCormick Observatory. Twenty years later, from subsequent studies at the Sproul Observatory, the location of the companion star was predicted, and the companion was both seen and photographed with the 200-inch Hale telescope at Palomar Observatory. A number of other perturbations have since been discovered at the McCormick Observatory and await further analysis. Various other aspects of double-star work were carried out under Mitchell's directorship.

In 1926 Mitchell initiated the taking of first epoch plates of Cepheids for the purpose of ultimately testing the zero point of the period luminosity curve. A similar program on long-period variable stars was also begun some time later.

The second area of Mitchell's activity was visual observation of variable stars. Again assisted by Olivier, Alden, and others, Mitchell observed a large number of long-period variable stars; with the McCormick telescope, stars down to the 15th magnitude or even fainter could be observed. This work included the determination of standard magnitudes for the comparison stars, used now by the American Association of Variable Star Observers. Mitchell also established visual photometric standards in selected areas of the sky.

Thirdly, Mitchell devoted considerable time to the observation of total solar eclipses. Between 1900 and 1934 he traveled to observe some ten total eclipses and obtained excellent photographs of several of them. He was particularly proud of his photograph of the "flash" spectrum of the chromosphere, taken when he was in Spain in 1905. With the collaboration of E. T. R. Williams and of Rupert Wildt, Mitchell derived important astrophysical information from these spectra. His book *Eclipses of the Sun*, published by the Columbia University Press, has gone through several editions.

Numerous other phases of astronomical work were started under Mitchell's directorship, such as Vyssotsky's discovery and study of nearby red dwarfs from objective-prism spectra taken with a 10-inch refractor. The same instrument has been used to supply spectral classifications of over 100,000 stars to several other observatories.

For several years, Mitchell was chairman of Committee A, on Academic Freedom and Tenure, of the American Association of University Professors and contributed to the work of this important committee.

Upon reviewing my 14 years' association (1923–1937) with Mitchell, I am impressed by the prodigious amount of work that was done under his directorship. Over the years, he gathered around him a number of dedicated astronomers who, operating on a small budget, with a minimum of secretarial and computational assistance, reached a vigorous rate of productivity. In addition to the enthusiasm of Mitchell and his co-workers, the fine quality of the 26-inch objective and the Virginia climate, one of the best for astronomical observation along the Atlantic seaboard, were factors in this productivity. That the small staff of the McCormick Observatory achieved so much was due, in no small measure, to the organizational qualities and the energetic directorship of Samuel Alfred Mitchell.

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Science in the News

Thinking about Disarmament: The Search for a Policy Turns Up Several Apparent Paradoxes

There has been a general increase in research on the problem of disarmament, dating from 1958 when the apparent progress being made at the Geneva test ban negotiations led to a feeling that the time might be right for at least some modest progress in other areas of arms control. The research has reflected a new and still evolving attitude among policy makers, who have tended in the past to regard disarmament negotiations as something which has to be undertaken for the sake of impressing the world with a nation's peaceful intentions but from which no real progress is to be expected. Interest in research on disarmament continues, despite the collapse of the Geneva disarmament negotiations and the present precarious state of the talks for banning nuclear weapons testing.

One fundamental difficulty in obtaining disarmament, as has been pointed out by Hans Morgenthau, of the University of Chicago, is that, without an international judiciary with the power to enforce its decisions, a nation is able to protect its interests, ultimately, only by the use or threat of use of military power. Consequently, disarmament can only follow, not precede, a relaxation of the tensions which have brought on an arms race. The value of such arms limitation agreements as might be reached is likely to be wholly illusory, as was, for example, that of the widely hailed Kellogg-Briand pact of 1928, which pledged its signatories to renounce war as an instrument of national policy.

Currently, research on disarmament is being conducted at the Rand Corporation, Santa Monica, California, which is a nonprofit organization supported primarily by the Air Force, and at several centers of international affairs at leading universities, with support by government and private grants. Until recently the program at Rand involved the part-time efforts of 12 men, but the research is being expanded.

The Traditional View

The goal of disarmament is part of the larger goal of seeking ways to reduce the likelihood of war. In the pursuit of this broader goal, the traditional emphasis has been less on disarmament and more on striving for a stable balance of power—that is, a balance in which no nation or alliance would be tempted to start a war since it would be opposed by an alliance strong enough to make the war extremely costly, strong enough to make the aggressor nation realize that there was a good chance it would lose the war. The trouble with the balance of power theory, according to some students of the problem today, is that nations do not behave as rationally as one would wish. A war can begin without either side really wanting it.

Present research on the problem of reducing the likelihood of war, although widely known as "disarmament research," differs chiefly from the traditional concern with the balance of power in replacing that concept with the concept of the balance of terror—that is, a balance among nations any of which has the power to pour devastation upon any other nation even if it is itself the victim first of a devastating surprise attack.

Some students of international affairs have now come to believe that a nation may find it in its own interest to work actively with its potential enemy to achieve a stable balance rather than seek only to maximize its own power and leave the potential enemy to keep pace. This school also believes that, given an apparently stable balance, rival powers may be able to work much more consciously than they have in the past to lessen the chance of war by accident, by misunderstanding, or by miscalculation.

What is being sought today under the heading of "disarmament research," accordingly, is not so much a decrease in the likelihood of war through disarmament as a decrease through an increase in international stability. In the short run at least, according to some students, serious proposals for reduction in armaments may play only a minor role in the effort to reduce the likelihood of war. This state of affairs is partly the result of the great difficulty of working out adequate inspection measures to insure that the provisions of a disarmament treaty are obeyed and partly the result of uncertainty concerning the effect of certain types of arms reductions on the goal of increasing international stability. In fact, it has been argued that disarmament, unless carefully planned, could, paradoxically, increase rather than decrease the danger of war.

Difficulties Facing Disarmament

It has been argued, for example, that the most promising areas for arms control today are not those which involve the dismantling of nuclear stockpiles and missiles; that, on the other hand, it is the very existence of massive and invulnerable deterrent forces that is the best safeguard against a nuclear war, since no nation can wish to start a war which it can be certain will lead to the inevitable devastation of its homeland. This view suggests that general nuclear disarmament may decidedly increase world tensions, since in the absence of an absolutely foolproof inspection system, which would probably be impossible to achieve, each nation would be stricken with fear that its rivals might be secretly assembling a nuclear striking force before which the nation observing the weapons ban would be defenseless. In any case, it can be argued that general nuclear disarmament, even if it could be achieved, would in one sense be illusory. For the technical knowledge of how to build bombs and missiles would still exist, and the outbreak or perhaps even the threat of outbreak, of war would be quickly followed by a race for nuclear rearmament; a nation might even be tempted to strike before the other fellow had a chance to assemble a nuclear force.

The weight of such arguments, plus the enormous difficulties of working out adequate and acceptable nuclear inspection systems, has led the researchers to think primarily in terms of stability-enhancing proposals with far more limited objectives than general disarmament and even to suggest that, in the short run at least, stability might be increased by additional armaments, including certain types of nuclear weapons.

The new research on "disarmament" involves attempts of political scientists, physicists, military experts, students of international law, and experts in still other fields to formulate proposals for promoting stability and to think them through carefully so that their implications may be understood as thoroughly as possible—H.M.

News Notes

Another Round in the Test-Ban Talks: The Pool Proposal

The United States has attempted to meet the Soviet demands for "safeguards" to insure that the U.S. seismic research program to improve methods of policing small underground nuclear explosions would not also serve to develop new weapons. At the Geneva test-ban talks on 12 July, James J. Wadsworth, the U.S. delegate, proposed the creation of a pool of early Western and Soviet nuclear devices that would be open to inspection and use by both sides.

This latest attempt to break the deadlock in the proceedings, which have been stalled for weeks over the method of carrying out a research program to improve existing seismic detection of small-scale underground tests, was acknowledged by the Soviet delegate, Semyon K. Tsarapkin, with a promise to refer the proposal to Moscow for study.

The terms of the proposal provide (i) that the three nuclear powers would supply approximately the same number of early nuclear weapons; (ii) that these devices would be inspected externally and internally (without destruction) by all three parties at the time of deposition; (iii) that the devices would be placed under international guard; and (iv) that each country could then withdraw from the pool devices to carry out research programs which had received the approval of the three parties. Each nation could use nuclear weapons of the other two nations for research purposes.

Soviet Reaction

Soviet reaction to the plan, while unofficial, has perhaps indicated the position that Moscow will take. Tsarapkin, addressing the summer school of the World Federation of United Nations Associations in Geneva on 19 July, did not object to the relaxation of American nuclear-secrets legislation to allow inspection of the test devices by the Soviet Union, but he denounced the proposal of international contribution to the pool as "fantastic" and "nonsensical." As the Soviet Union is perfectly satisfied with the existing detection system and intends to conduct no research, he asserted, there is no

reason for the Soviet Union to contribute devices to such a pool; the United States, however, definitely should allow inspection of those weapons which will be used in the proposed research program. The Russians have asserted that without Soviet inspection the United States would be able to test illicit weapons.

Tsarapkin used this opportunity to repeat charges that "influential forces" in the U.S are blocking the signing of a treaty banning all nuclear tests. Sir Michael Wright, the British delegate, who addressed the summer-school group after the Soviet delegate had concluded his remarks, dismissed the Russian's speech as "propaganda." Wright had indicated British approval of the latest American plan at the time of its proposal.

Secrecy Legislation

In proposing the plan, Wadsworth announced that the Administration would ask Congress to relax existing secrecy legislation to allow the plan to become operative. (Relaxations would apply only to devices for seismic research programs.) The Administration first broached the idea of Soviet inspection of American nuclear devices to the Joint Committee on Atomic Energy on 24 June. The adverse reaction of the committee led to the resubmission of the proposal three days later with a clause providing for reciprocal inspection.

A member of the committee, Bourke B. Hickenlooper (R.-Iowa) has stated that he sees no objection to reciprocal inspection of "obsolete" nuclear devices. Other political sources have questioned the Administration's ability to pass such legislation during a campaign year.

The purpose of the nuclear-secrets legislation has been twofold: (i) to prevent information from falling into Soviet hands and (ii) to keep non-nuclear nations from obtaining material which would assist them in developing atomic weapons. The former concern now applies only to the more advanced weapons, but the latter involves the older weapons as well. The question has been raised in some political circles as to how the United States could share such information with the Soviet Union and refuse to make it available to U.S. allies.

Existing legislation empowers the executive branch to tell our allies only about the external characteristics of our nuclear devices so that U.S. warheads can be fitted on allied delivery systems. Great Britain is an exception. Under the terms of the Atomic Energy Act of 1954 no country can be told of the weapon design or nuclear components of these devices, but a 1958 amendment to this law enabled the executive branch to give this information to Great Britain.

Other Developments

Earlier this month, in another development, the Russians agreed to a British plan for East-West parity in top jobs on the international control board which would supervise the moratorium on nuclear testing. This plan provides that the chief administrator and his principal deputy will be neutrals acceptable to both sides. The four other deputy-administrator positions will be divided equally between the East and the West.

The day after the pool plan was submitted, Wadsworth announced that the United States agreed to the idea of a fixed number of inspections for the parties to the treaty. The treaty proposals still do not specify the number of such inspections, but informally the United States has suggested 20 inspections for the Soviet Union. Wadsworth again urged Tsarapkin to state the number of inspections that would be acceptable to the Russians. Tsarapkin continued his 20-month silence on this question.

The U.S. proposal also outlined the following procedure for establishing the number of inspections for nations that would sign the treaty in the future. In the beginning each new country would agree to one inspection per year for each 500,000 square kilometers; as the control commission gained experience, quotas of no fewer than two a year would be fixed for each country. Any party to the treaty would have the right to demand inspection in countries other than the original three, within their respective quotas.

The United States, in advancing the pool proposal and in agreeing to the idea of a fixed number of inspections, has set a new test of Soviet intentions as regards the test-ban treaty. Washington has stepped up the pace of the Geneva discussions to find out if Moscow intends to sign the treaty or if the Soviets are determined to keep the negotiations open indefinitely.

Pituitary Bank Founded

The establishment of the first pituitary bank in the world has been announced by the University of California Medical Center in San Francisco. Roberto F. Escamilla, clinical professor of medicine, will direct the bank, which will supply human pituitary glands for the treatment of children stunted by a deficiency of growth hormone. The need for such a bank has been demonstrated by research conducted in the medical school's Hormone Research Laboratory on the Berkeley campus.

Establishment of the bank is being aided by a grant from the Jefferson McLeod Moore Fund of the School of Medicine. Clinical studies have been supported by the Lasker Foundation, the American Cancer Society, and the McKee Fund of the School of Medicine.

Donations to the bank are to be made in the same manner as donations to eye and artery banks. Donors will sign an authorization for removal of their pituitaries at the time of death and will inform their relatives and physicians of this decision. The special authorizations must be filed with the university. Because the pituitary must be removed soon after death, such a donation cannot be accomplished by a clause in one's will. Authorization forms and additional information can be obtained from the University of California Pituitary Bank, University of California, San Francisco Medical Center, San Francisco 22, Calif.

Malaria Eradication

The following is the text of a statement released by the World Health Organization during July. The transmission of a vivax*-type malaria parasite of macaques to man was reported by D. E. Eyles et al.* [Science **131**, *1812 (17 June 1960)*].

It has long been known that the higher (anthropoid) apes had malaria, and that man might be susceptible to certain malaria parasites of apes. Opportunities for the transmission of malaria from apes to man are exceedingly rare, and the problem has therefore little practical importance.

The lower monkeys are also naturally infected with several types of malaria, but these are not transmissible

to man. Quite recently, however, laboratory research has shown that one of the malaria parasites of these monkeys can be transmitted to man by the bite of an anopheline mosquito which has fed on the monkey's blood.

A "reservoir" of malaria in monkeys may conceivably, therefore, add to the difficulties of eradicating human malaria in certain countries in which monkeys live in close proximity to man.

It is important, however, not to exaggerate the gravity of this situation, which is similar to that of diseases such as yellow fever, filariasis, or rabies where wild-life reservoirs of infection exist. Ways will have to be found of overcoming the difficulty, just as ways were found to overcome other difficulties in malaria eradication campaigns, such as the resistance of certain mosquitoes to insecticides.

Moreover, the monkey-malaria problem is likely to present itself only in a few areas of some countries of east Asia, while the eradication campaigns will be unaffected elsewhere.

Indeed, the new discovery is a cause for rejoicing rather than for concern. From the research point of view it is an advantage to be able to work with a malaria parasite which causes the disease in man but which can also be studied in monkeys. This may indeed open vast possibilities for experimental studies on new anti-malarial drugs which may hasten the final conquest of malaria.

Seismographic Facilities Expanded

Studies of earthquake aftershocks, which may be of value in developing techniques to distinguish between small natural earthquakes and man-made explosions, are being planned by the University of California, Berkeley. Six new seismographic facilities will be built in central California to automatically transmit earthquake information to the central seismographic station on the Berkeley campus.

The new stations will transmit information, by means of a telemeter, to the Berkeley station for recording. Under such an arrangement, (i) the outlying stations will not have to be staffed, (ii) only one timing system (that at Berkeley) will be required, and (iii) instrument troubles at the other stations will be immediately detected at Berkeley through the telemeter system.

The information system will enable the Berkeley scientists to locate the epicenter of a small earthquake (below magnitude 5) within a period of 1 or 2 hours after the main shock. After the epicenter has been located, three mobile seismographic units will be sent to the area and arranged to record the maximum possible amount of data on the aftershocks. These truck-based seismographic units will be equipped with radio-telephone transceivers so that placement and relocation can be supervised by the Berkeley staff. The project will also include the construction of a subterranean station to house the seismographic instruments of the Berkeley station.

Perry Byerly, professor of seismology and director of the university's seismographic stations, and Don Tocher, research seismologist, will direct the project, which is supported by a $400,000 grant from the Air Force Office of Scientific Research.

News Briefs

Sunspot photograph. A stratoscopic photograph of the umbra of a sunspot, taken from a balloon-borne telescope, shows clearly, for the first time, white gaseous dots within the umbra. The photograph just released by the National Science Foundation, was taken last September, near Minneapolis, by the Princeton University astronomy team, headed by Martin Schwarzschild. The bright spots are small in relation to the umbra—200 miles in diameter as compared to 8000 miles—and their lifetime is about half an hour. The nature of the spots is uncertain, but Robert Danielson, a member of the Princeton team, believes that they may be "convection cells somewhat like solar granulations, but strongly suppressed by the magnetic field of the sun spot."

* * *

Polio vaccine trial. The Yale Poliomyelitis Study Unit recently completed the main phase of the Sabin live poliovirus vaccine trial, which started in New Haven last February. All of the 350 participants have now received the full course of vaccine for 1960. During the trial period there were no illnesses or any bad effects which could be attributed to the vaccine.

However, a long period of laboratory research is now necessary to determine how effective the vaccine has been, according to John R. Paul, professor of preventive medicine at Yale and director of the work. The Yale Poliomyelitis Study Unit has had experience with the disease for more than 30 years, and its laboratories are specially equipped to handle major projects such as the field trials.

* * *

Italian scientists strike. This month Italy's nearly 2000 nuclear scientists went on strike for 2 days to protest (i) the interference of political and private business interests in their work and (ii) government inertia on research projects. Laboratories closed and physics students were not examined. The scientists hope to obtain a long-term commitment from the government to provide funds for research.

* * *

Mt. Allison Institute. The department of extension of Mt. Allison University, Sackville, New Brunswick, Canada, is sponsoring a public affairs project, the Mt. Allison Summer Institute, 12–14 August. Under the general topic of "Science and Society," the institute will discuss science and human values, science and western culture, science and nationalism, the peaceful uses of atomic energy, the scientist and social responsibility, and the place of science in our educational system.

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AAAS branch anniversary. The Mobile Academy of Science, a local branch of the AAAS, celebrated its 25th anniversary on 20 May with a dinner at which the principal speaker was Fred R. Cagle, coordinator of research at Tulane University. Cagle, who has made two trips to the U.S.S.R. in recent years, spoke on "Science in the Soviet Union." The new president of the academy is James Boyles, University of Alabama Mobile Center.

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University reactors. The nuclear reactor on the university campus will be the subject of a meeting to be held in Gatlinburg, Tenn., 17–19 August, under the sponsorship of the Oak Ridge National Laboratory and the Oak Ridge Institute of Nuclear Studies, in cooperation with the Oak Ridge section of the American Nuclear Society and the Atomic Energy Commission. Further information is available from the University Relations Division, Oak Ridge Institute of Nuclear Studies, Box 117, Oak Ridge, Tenn.

New Journals

Katunob, Newsletter on Mesoamerican anthropology, vol. 1, No. 1, May 1960. Box 87, Southern State College, Magnolia, Ark. Irregular. $1.

The Mankind Quarterly, June 1960. R. Gayre, Ed. 1 Darnaway St., Edinburgh 3, Scotland. Quarterly. $4.

Analytical Biochemistry, vol. 1, No. 1, June 1960. A. Nason, Ed. Academic Press, New York, N.Y. vol. 1 (6 issues) 1960. $15.

Botanica Marina, vol. 1, fasc. 1 and 2, 1959. Cram, De Gruyter, Hamburg, Germany. DM. 28.

ICSU Review, vol. 1, No. 1, Jan. 1959. H. S. Jones, Ed. Elsevier, Amsterdam, Netherlands. Quarterly. $4.50.

Journal of Reproduction and Fertility, vol. 1, No. 1, Feb. 1960. C. R. Austin, Ed. Blackwell, Oxford, England. Quarterly. $12. Proceedings of the Society for the Study of Fertility are now published in this journal.

Radiological Health Data, Apr. 1960. U.S. Department of Health, Education, and Welfare. Office of Technical Services, U. S. Department of Commerce, Washington 25, D.C. Monthly. $0.50 per issue.

Compost Science, vol. 1, No. 1, spring 1960. J. Olds, Ed. Rodale Press, Emmaus, Pa. Quarterly. $4.

Medical World News, vol. 1, No. 1, Apr. 1960. M. Fishbein, Ed. Medical World Publishing Co., New York, N.Y. Biweekly. $12.50.

World Neurology, journal of the World Federation of Neurology, vol. 1, No. 1, July 1960. C. M. Poser, Ed. 84 S. 10th St., Minneapolis, Minn. Monthly. $15.

Ospedali d'Italia Chirurgia, vol. 1, No. 1, Nov. 1959. T. Greco, director. Via M. Buonarroti, 17, Florence, Italy. Monthly. $18.

Cancer Chemotherapy Abstracts, vol. 1, No. 1–2, Jan.–Feb. 1960. Scientific Literature, Inc. (108 S. 22nd St., Philadelphia 3, Pa.), for Cancer Chemotherapy National Service Center, Bethesda, Md. Monthly.

Developmental Biology, vol. 1, No. 1, Apr. 1959, M. V. Edds, Jr., Ed. Academic Press, New York, N.Y. $14.

Genetical Research, vol. 1, No. 1, 1960. E. C. R. Reeve, Ed. Cambridge University Press, New York, N.Y. Three issues per year. $17.50.

Ciencia Interamericana, vol. 1, No. 1, Jan.–Feb. 1960. Panamerican Union, Washington, D.C. Bimonthly. $0.15 per issue.

Scientists in the News

Midwesterners won three of the nation's highest awards in engineering education during the recent annual meeting of the American Society for Engineering Education at Purdue University.

Robert B. Bird, professor of chemical engineering at the University of Wisconsin, received the $1000 George Westinghouse Award for outstanding teaching ability in a young engineering faculty member.

Maurice J. Zucrow, director of the jet propulsion laboratories at Purdue University, received the Vincent Bendix Award for outstanding research.

Ali B. Cambel, chairman of the mechanical engineering department at Northwestern University, received the $1000 Curtis W. McGraw Award for research contributions by a young teacher of engineering.

The fourth award, the Lamme gold medal for distinguished contributions to engineering education and research, went to **Theodore von Karman**, chairman of the Advisory Group on Aeronautical Research and Development for NATO, who addressed the 3000 convention delegates.

Three of the four ASEE award winners were born abroad—von Karman in Hungary, Zucrow in Russia, and Cambel in Turkey—but all except the first received part of their education in the United States.

Maxim K. Elias, emeritus professor at the University of Nebraska, has received a National Science Foundation grant to spend 2 years at the Research Institute of the University of Oklahoma to work on the program "Carboniferous Bryozoa of America and Europe." He will make a trip to Europe during the second year of the grant.

This summer he will attend the International Geological Congress at Copenhagen as a member of the subcommission on carboniferous stratigraphy. He plans to lecture in the U.S.S.R. (in Russian) in September. He also expects to visit scientists in Leningrad and Moscow to facilitate the exchange of literature and scientific specimens in the areas of geology and paleontology.

Floyd S. Daft, director of the National Institute of Arthritis and Metabolic Diseases, National Institutes of Health, has been elected president of the American Institute of Nutrition, effective 1 July. He will serve a 1-year term, during which the American Institute of Nutrition will be host society to the fifth International Congress on Nutrition, to be held in the United States this year for the first time. Approximately 3000 nutrition scientists from all parts of the world will attend the meeting in Washington, D.C., which is scheduled for the week of 1 September. Upon completion of his presidency in 1961, Daft will serve as chairman of the executive committee of the Federation of American Societies for Experimental Biology.

Jerome G. Rozen, Jr., assistant professor of entomology at Ohio University, has been made associate curator in charge of entomology at the American Museum of Natural History, New York.

Willis E. Lamb of Oxford University, who shared the 1955 Nobel Prize in physics with Polycarp Kusch, professor of physics at Columbia University, will return to Columbia as visiting professor of physics during the spring term of the 1960–61 academic year. He is a former member of the Columbia faculty.

Robert A. Rubega, a physicist in the underwater acoustics section of Stromberg-Carlson's Research Division, has received the first annual Stromberg-Carlson Award for Science and Technology. The $2000 award, established this year by the Stromberg-Carlson Division of General Dynamics Corporation, is to be given annually to an employee in recognition of outstanding achievement in science and technology.

Paul A. Anderson has resigned the chairmanship of the department of physics at Washington State University, Pullman, to devote more time to his research in biophysics. The new department chairman will be **William Band**, professor of physics at Washington State. He will assume his new post on 15 September.

James B. Kendrick, Sr., retired on 1 July after 33 years of service with the University of California. At retirement he was professor of plant pathology and plant pathologist in the California Agricultural Experiment Station, Davis. He had been in charge of the department of plant pathology on the Davis campus since 1927 and

chairman of the Davis-Berkeley departments of plant pathology since 1954.

Lysle D. Leach succeeds Kendrick as chairman of the Davis-Berkeley departments, and **W. C. Snyder** continues as vice-chairman.

The following men have been reappointed to the National Science Board.

The Rev. **Theodore M. Hesburgh**, president of the University of Notre Dame.

William V. Houston, president of the Rice Institute.

Joseph C. Morris, vice president of Tulane University.

The new appointees to the board were announced in the 15 July issue of *Science*.

Frederick J. Dockstader, assistant director of the Museum of the American Indian, Heye Foundation, New York, has been named director. He will succeed **E. K. Burnett**, who becomes director emeritus following a period of 30 years' service to the museum.

Keith R. Porter, who conducted pioneering research on cellular structure with the electron microscope, will become professor of biology at Harvard University on 1 July 1961. Until then, he will continue his research at the Rockefeller Institute, where he has been professor since 1956.

Alphonse Chapanis, professor of psychology and industrial engineering at the Johns Hopkins University, is taking a year's leave of absence, which he will spend as psychologist on the staff of the Office of Naval Research Branch Office in London. He will visit university, military, and industrial laboratories, attend scientific meetings, and give lectures throughout western Europe. His address will be: Office of Naval Research Branch Office, Keysign House, 429 Oxford St., London W.1, England.

Harold A. Scheraga, professor of chemistry at Cornell University and a leading investigator in the field of proteins, has been appointed chairman of the university's department of chemistry. He succeeds **Franklin A. Long**, who has completed his second 5-year term as department chairman.

Ernst W. Caspari, developmental geneticist and formerly chairman of the department of biology at Wesleyan University, has been appointed chairman of the department of biology at the University of Rochester, effective in September.

Arnold W. Ravin, former department chairman at Rochester, has been promoted to associate dean of the College of Arts and Science. During the academic year 1960–61, Ravin will be on sabbatical leave conducting genetic research, part of the time at the Institute de Génétique, Gif, France.

Heinz Werner, G. Stanley Hall professor of genetic psychology at Clark University, is retiring from the faculty and from the chairmanship of the university's department of psychology. He will continue in his posts of director and board chairman of the Institute of Human Development, an adjunct of the Clark department of psychology. He will also continue his grant-sponsored research in perception and language and will teach a graduate-level seminar.

George M. Briggs, executive secretary of the Biochemistry and Pharmacology Training Committees, Division of General Medical Sciences, National Institutes of Health, has been appointed chairman of the department of nutrition in the College of Agriculture at the University of California, Berkeley.

Oscar W. Richards, chief biologist of the American Optical Company's Research Department, Southbridge, Mass., has been elected an honorary fellow of the Royal Microscopical Society.

Paul W. Edwards, a chemical engineer with almost 40 years of service with the U.S. Department of Agriculture, has retired from the Agricultural Research Service's Eastern Utilization Research and Development Division in Wyndmoor, Pa. Edwards has been a member of the staff of the Wyndmoor laboratory since the first day of its operation, 17 August 1940, and has played a prominent role in many of its outstanding engineering developments. He has been in charge of unit operations investigations in the division's Engineering and Development Laboratory.

The American Museum of Natural History has announced several appointments.

William K. Emerson, formerly assistant curator in the Department of Fishes and Aquatic Biology, has been named chairman and assistant curator of the newly established Department of Living Invertebrates.

Norman D. Newell is chairman and curator of the newly established Department of Fossil Invertebrates. Since 1945 Newell has held positions both as curator of historical geology and fossil invertebrates at the Museum and professor of invertebrate paleontology at Columbia University.

Brian H. Mason, curator of physical geology and mineralogy, has been named chairman and curator of the newly established Department of Mineralogy.

Merrill S. Read will become director of nutrition service for the National Dairy Council, Chicago, on 15 August. He succeeds **Zoe E. Anderson**, who resigned on 1 June to become head of the department of home economics at Wayne State University. Read joins the Dairy Council from Virginia Polytechnic Institute, where, since last year, he has been visiting professor in the department of biochemistry and nutrition.

Evelyn L. Oginsky, associate professor of bacteriology at the University of Oregon Medical School, has received the 1960 Research Award of Iota Sigma Pi, national honor society for women chemists. The award is made once every 3 years to "recognize distinct achievement and to encourage a continuing and expanding career of greater promise." It has been presented only four times previously. Dr. Oginsky, co-author of the textbook *Introduction to Bacterial Physiology*, is widely recognized for her study and analysis of mode of action of streptomycin.

H. F. Stimson, a senior physicist in the National Bureau of Standards' Heat Division, has retired after more than 42 years of service. Stimson has made many contributions in the field of heat measurement and is recognized as one of the world's foremost authorities on the International Temperature Scale.

Ismael Vélez, professor of botany and director of the National Science Division of the Inter American University in San German, Puerto Rico, will lecture in botany at the University of Cuzco, Peru, from August through December.

Erratum. On the contents page of the 22 July issue, the authors of the report "Sound production by the satinfin shiner, *Notropis analostanus*, and related fishes," were incorrectly listed. H. E. Winn and J. F. Stout wrote the paper.

Book Reviews

Beyond the Welfare State. Economic planning and its international implications. Gunnar Myrdal. Yale University Press, New Haven, Conn., 1960. xiii + 287 pp. $4.50.

This book presents a simple, three-step thesis: (i) the democratic countries are moving, by necessity, towards the welfare state; (ii) welfare state planning has become, by necessity, nationalistic, and has contributed to international economic disintegration; (iii) despite all past failures and expected future difficulties, a new effort at international cooperation through supranational planning must be attempted.

In advancing this thesis Gunnar Myrdal uses economics, political science, and social psychology, but beyond that every page, in one way or another, reflects his personal experience. Myrdal does not look at his topic in the detached manner of the ivory-tower scholar. As a leading Swedish expert on political economy and, at times, an active political leader, he was one of the architects of the Swedish welfare state. For a decade he had the often frustrating task of guiding the staff work of a United Nations agency for Europe (embracing East and West European countries). In recent years he has become thoroughly familiar with the struggle and plight of the Southeast Asian countries; his wife is the Swedish ambassador to India, and he directed a research project on Southeast Asian countries for the Twentieth Century Fund.

It is fascinating to see how Myrdal has combined the varied experiences of his life into a consistent structure of thought. The reader cannot help being impressed by his sincerity, intellectual integrity, and sober idealism. However, the fact that Myrdal looks at political, economic, and social world events, from the perspective of his own experience, in such an outspoken manner, also explains certain oversimplifications and exaggerations in his picture.

His picture of the welfare state is very much painted after the Swedish model. Interesting is Myrdal's observation that manifold and often contradictory government interventions and regulations characterize only the early stages of development in the welfare state. With advances in planning by public and private organizations, a "debureaucratization" can take place and voluntary organizations become of growing importance. In the last analysis, he regards the welfare state as "a system of social morals" (page 95) and speaks of the ideal of a "Welfare Culture." [Incidentally, he forgets his own thesis when he says that the "Welfare State . . . is an increasingly regulatory one" (page 168)].

He observes that active citizen participation, particularly in running local government and voluntary organizations—for example, in unions and business organizations, is less intensive in the United States than elsewhere (read: Sweden). He offers as the explanation of this deficiency in the United States—the remnants of separatistic allegiances in a country populated largely by immigrants. He is, however, very optimistic concerning "the rise in efficiency, democratic control and honesty, particularly in the private power groups and the authorities for provincial and municipal self government, where the deficiencies were biggest and are still considerable" (page 55).

Myrdal is merciless in emphasizing that welfare policies are responsible for the international economic disintegration. I believe he exaggerates this point. Disintegration of the international economic system of the 19th century had its origin in conditions much older and broader than welfare state policies. However, it is true that modern policies were not very successful in establishing a well-working, world economic system. Examples given by Myrdal include: the failure to stabilize international prices of primary products (in contrast with actions taken to stabilize domestic agriculture and mineral prices); the failure to stimulate a flow of private capital from highly developed to under-developed countries; the failure to organize *international* programs of foreign aid (in contrast with programs that are sponsored mainly by the United States) of adequate scale and efficiency.

His explanation is that "the international and common interests have no organized pressure groups in the protective Welfare State of the rich countries . . ." (page 243).

I am surprised that Myrdal overlooked the fact that a primary objective of the modern state (what he calls the welfare state) is the maintenance of full employment and the mitigation of violent economic fluctuations. This domestic policy is also the most effective support for maintaining a high level of imports. It is in striking contrast with the policy of "exporting unemployment" of a generation ago.

In Myrdal's regret about the lack of international planning, he plays down too much the significance of regional organizations such as the European Common Market. (We remember that he is a Swede). While Myrdal does not see the value of regional arrangements of industrial countries, he believes the underdeveloped countries should combine. He appears at times to advocate: "underdeveloped countries of the world unite; you have nothing to lose but your —markets!" What hope Myrdal has for future reconciliation between the nationalistic welfare state and the new world-order policies is not based primarily on the political pressure of the underdeveloped countries, but on his deep belief that "people at bottom, behind the facade of nationalistic attitudes, do believe in, and do desire, international cooperation" (pages 277 and 279). Domestically the task is to procede from the regulatory phase of the welfare state to what he calls a "Welfare Culture"; internationally to procede from the present nationalistic phase of the welfare state to an international "Welfare World." Both tasks of reaching "Beyond the Welfare State" are difficult but not hopeless. However, he believes that the political and psychological obstacles in the international field are much more serious than those that tend to obstruct the accomplishment of the domestic task.

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Measuring the Invisible World. The life and works of Antoni van Leeuwenhoek. A. Schierbeek. Abelard-Schuman, New York, 1959. 223 pp. $4.

The author of this book, A. Schierbeek, is editor-in-chief of the 280 or so letters of Antoni van Leeuwenhoek (1632–1723), the collecting of which is still in progress. *Measuring the Invisible World* is a useful book that adds much to our knowledge of Leeuwenhoek and gives us an evaluation of his contributions to scientific knowledge. It constitutes a major step toward realization of the author's anticipation that "in the year 2000 we shall have a much better understanding of Leeuwenhoek than we have today."

Leeuwenhoek, who never wrote a book, entrusted the recording of his observations and discoveries to his letters alone. Because one letter often embraces more than one topic and, in addition, the same subject may appear in several letters as research advances, the task of Schierbeek was not an easy one, especially in his endeavor to present a "true impression of Leeuwenhoek's investigations." His method was systematically to gather together and translate into English adequate samples of Leeuwenhoek's writings, classifying them into categories. Then under each topic he critically studies and comments upon the subject matter, thus making accessible a vast amount of hard-to-get and hard-to-organize material. As a result, we have a more accurate picture of Leeuwenhoek, the central figure of this book.

The general order of Schierbeek's classification, from chapters 3 through 9, falls under the following main topics: In chapter 3 are considered Leeuwenhoek's microbiological investigations, which caused a great sensation in scientific circles; his finding of the "animalcules" in a great variety of specimens; his original research that led to the discovery of the unicellular organisms, the Protozoa, as well as his pioneering and almost startling notes on the observation of bacteria. Chapter 4, on the theory of generation, discusses his views on the ovarian structure of many species (human ovum, however, was not discovered until 1827, when it was announced by Carl Ernst Von Baer), and his discovery of spermatozoa, which formed a step toward making embryology a science. Such investigations show why Leeuwenhoek became a vigorous opponent of the doctrine of spontaneous generation, which was prevalent from antiquity up to the 17th century, and why he embraced his preformistic interpretation of the origin of spermatozoa. This chapter also reports his description of the minute fresh-water creatures: the formation of the daughter colonies in volvox, the budding of the hydra and the rotifers. Chapter 5 presents Leeuwenhoek's fascinating investigations in histology: red blood corpuscles and his interpretation of their origin; capillary circulation which Malpighi had discovered earlier; and his pioneering microscopic observations of the tissues of nerves, muscles, and the lenses of the eyes of many animals, as well as of bones, teeth, and hair. Then, in a most elegant manner, the author displays in chapter 6 Leeuwenhoek's careful entomological reports on the plant louse, the grain moth, the flea, and the "compound" eyes of insects.

Chapter 7 deals with Leeuwenhoek's investigations of lower animals, while chapter 8 is concerned with his writings on wood structure, the stings of nettles, the transport of nourishment in plants from "globule to globule," and the seeds of plants which are contained in the "embryos." Chapter 9, on medicine, shows Leeuwenhoek's interest in therapeutic observations and his liberty in criticizing both the quackery and the irrational procedures found in medical practices during his time. It also contains his remarks on crystals and salt in plants. The concluding chapter makes a brief survey of the contributions of this immortal scientist from Delft, his place in the period during which he lived, and his impact upon his contemporaries and upon later generations.

Of special interest to the historian of science is the informative and well-organized material in chapter 2, which serves as the author's introduction to his systematic studies in the chapters that follow. Chapter 2 contains a brief history of microscopy and the method by which this great microscopist constructed his some 550 single-lens microscopes, using the simple magnifying glass; a description of his use of a counter in the earliest known attempt to measure the size and estimate the number of examined microscopic objects; and his first letter (May 1673) to the Royal Society of London.

Chapter 1, written by Maria Rooseboom, is a biography of Leeuwenhoek. It includes an account of the noteworthy events during the illustrious days of the Dutch Republic, the role played by the middle class, and the new philosophy that spread among learned men of the 17th century who sought to understand the universe by knowing the laws which govern it. Such a historical and geographical setting is especially appropriate for understanding a man who spent almost all his life in the land of his birth and who, in spite of his diligence in research, never knew any language other than his mother tongue. Leeuwenhoek, nevertheless, we are told, enjoyed good relations with the Royal Society and was generous to his visitors, among whom were dignitaries and eminent men who came to Delft from many parts of the Western world to pay homage.

In her brilliant approach and persuasive style, Maria Rooseboom gives us profound insight into the intellectual activities of Leeuwenhoek within his environment. She helps us not only to become acquainted with his life but also enables us to appreciate more completely how the aims of the book are ably fulfilled in the chapters that follow. I regret, however, that her narrative-chronicle has repeated and unnecessary interruptions that somewhat disturb both the reader's concentration and the harmony of the contents (see, for example, pages 15–25). She also overemphasizes several ideas and observations, later mentioned in the main text, in her endeavor to explain their importance; as a result, when they are expounded in detail by Schierbeek under the appropriate topic, their appeal is minimized. Neither Rooseboom nor Schierbeek has escaped the temptation to give an extra measure of praise to their renowned countryman whom they both love and highly admire. This explains, moreover, why they insist on attributing to Leeuwenhoek, though often with much justification, the title of being the first, or the father, of a science or of a discovery.

Although in my estimate (especially in regard to the author's aims) chapters 1 and 6 constitute the most efficiently handled parts of the book, by and large Schierbeek has done great justice to his subject in other chapters as well. His preludes under the various classified categories are again of particular interest to the historian of science, for they link together earlier developments of a subject with the degree of its advancement in Leeuwenhoek's time. Therefore, it does not diminish in any way our

large debt to an able author to say that more adequate coverage is needed for a number of the topics discussed in these surveys—for example, in the prelude on man's knowledge from antiquity to the 17th century of blood circulation. Here we might expect the author at least to mention in passing the improvement made in the understanding of blood circulation, because this understanding differed from the Galenic interpretation and was expressed 4 centuries prior to the monumental experiments and resolutions of William Harvey. Also, the attempt to connect Sarton's comment on Leonardo da Vinci with Leeuwenhoek's achievement seems artificial, because the circumstances that led da Vinci to write his notes for "his own private use" were completely different from those of Leeuwenhoek. In fact, it seems in several passages that Schierbeek tries to put words into Leeuwenhoek's mouth by interpreting his ideas and modifying the interpretations to fit modern scientific concepts. Such ideas, it is fairly safe to say, were in all probability not so clearly conceived in the mind of our 17th-century scientists. For the same reason we might have hoped that Schierbeek would translate Leeuwenhoek's own words on the axons of nerve fibers and their structure, as he faithfully does other passages, rather than give us Cole's summary.

A minor criticism is in regard to the language of the book. Since English is not my native tongue nor that of the author, I am more appreciative of the difficulties faced in avoiding weak sentence construction, and from this point of view, I am sure that the author has secured a greater measure of success. Nevertheless, here and there a few grammatical errors and awkward sentences do appear. They are not serious mistakes, but they could be corrected. Up to a point, however, originality of expression in several passages rendered the discussion more interesting and genuine.

In concluding, I must state that this book, which deserves large credit, has given us a remarkable presentation of the intellectual activities of an immortal scientist. A close study of it will repay not only those concerned with the history of biological studies but also other historians of science who are interested in scientific progress in the early modern period.

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Medizinische Grundlagenforschung. vol. 2. K. Fr. Bauer, Ed. Thieme, Stuttgart, Germany, 1959 (order from Intercontinental Medical Book Corp., New York). viii + 827 pp. Illus. DM. 168.

Volume 2 of the scheduled series, *Medizinische Grundlagenforschung* (Fundamental Medical Research Problems), continues the tradition of volume 1 and presents the acute borderline problems of medicine and natural sciences in a form which, although apparently heterogeneous, nevertheless tends to assist in finding a synopsis for the multiplicity of modern scientific life. This main purpose of the series has been achieved with volume 2. In 17 chapters it deals with such problems as the formation of structure in nature and the second law of thermodynamics, cellular theory, cell electrophoresis, the importance of fibrinolyses, the chemotherapy of tumors and skin diseases, cancerogenic substances, and others. The presentation is clear and concise with the author's original work skillfully blended into the general review of the problems. Numerous, well-selected references encourage further studies and research.

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Photographic Lunar Atlas. Based on photographs taken at the Mount Wilson, Lick, Pic du Midi, McDonald, and Yerkes Observatories. G. P. Kuiper, Ed. University of Chicago Press, Chicago, Ill., 1960. $25.

This atlas is a magnificient achievement, prepared with loving care. I well remember my astonishment, while spending a day at the Lick Observatory in June 1957, at finding Kuiper in the darkroom making some of the prints which are now published.

Planning for the project began some 5 years ago. Fine photographs of the moon were already in existence at a number of the major observatories, and these were the source material for the volume. In August 1955 the proposal was discussed at the Dublin meeting of the International Astronomical Union, and the work of copying the negatives was begun in 1956. From some 1200 prints, final selection of the prints to be used was made late in 1959.

The prints are on a scale of 1:1,370,000, which corresponds to a lunar diameter of about 100 inches. On this scale a mile equals 1.2 mm. A supplement is planned, on double the scale, giving high-quality photographs of selected objects.

The atlas is divided into three parts: (i) an introduction (11 sheets), showing the subdivision of the lunar surface into 44 fields, and so forth; (ii) the main body (184 sheets); and (iii) 35 supplementary sheets. The total number of sheets is 230 and each sheet measures 17 by 21 inches.

In the main section, 176 sheets are printed in sets of four and folded. This makes them extremely awkward to handle in normal office use. These 34 by 42 inch sheets will doubtless be even more awkward to handle at the telescope. It seems unlikely that the paper will survive more than a few refoldings without cracking. Most users will probably decide to cut these sheets.

The atlas was planned for use at the telescope, and the type of reproduction and the paper were selected to permit the charts to be retouched at the telescope with pencil, crayon, or ink. Kuiper recommends "that observers use at least two copies, one for reference and the other(s) as research material at the telescope. The cost of the atlas has been held to a minimum, roughly 10 cents a sheet, not much more than drawing paper." It would be a real service to observers if replacement sheets could be purchased separately.

Packaging anything as heavy and bulky as this volume always presents problems, and these problems have not been completely solved in the present case. The box containing the review copy started coming apart shortly after it was received, and it is obvious that it is not sturdy enough to be used for library storage and handling. The charts could be kept in a table drawer, or they could be stored vertically in a standard x-ray file cabinet. Such files have been used very successfully for the *Palomar Sky Atlas*, and those who own that atlas might have enough space left for the *Lunar Atlas*. A more durable box could be made by having an inner box that slides into an outer box with the openings on the 2½ by 22 inch side. A more durable holder should be seriously considered when future editions are prepared.

Kuiper's introduction states that "The purpose of this atlas is to present the surface record of the moon as

shown on the best photographs now available. . . . Not only do the best photographs show, on the whole, more than the visual maps; they show the lunar surface with precision, both as to location and intensity and, of course, with completeness up to the resolving power attained, none of which are practicable in visual drawings. The information is increased if several photographs of the same area taken under different illuminations are combined to bring out the maximum advantage both low level detail and steep mountain slopes." Kuiper and his collaborators have achieved this purpose fully, and they deserve the gratitude of astronomers and others who are interested in the lunar surface.

The initial financial support for the project came from the National Science Foundation. Later support was provided by the Cambridge Research Directorate of the Air Force.

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Ticks. A monograph of the Ixodoidea. Part 5. On the genera *Dermacentor, Anocentor, Cosmiomma, Boophilus,* and *Margaropus.* Don R. Arthur. Cambridge University Press, New York, 1960. xviii + 251 pp. Illus. $11.50.

Acarologists will welcome the resumption, after 34 years, of this series of monographs on Ixodoidea and the announcement that the final volumes, Part 6 and Part 7 on other genera are in preparation. This pleasure, however, will be tempered by the fact that, compared to its more scholarly predecessors, this is an obviously inferior treatment, despite the very adequate descriptions and illustrations of the 35 species included. Keys for differentiation of genera—for example, *Margaropus* from *Boophilus*—and introductory reference to illustrated differences of Sections Prostriata and Metastriata in Part 2 (1911) would have been helpful. Under genus *Dermacentor* (page 6), the author's opinions are provided for neither subgeneric nor synonymic status, nor are there adequate references to type species, of *Conocentor, Indocentor, Amblyocentor,* and *Puncticentor* Schulze (pages 7, 137, 153, 172, 189), though he uses, without explanation, one in the heading *D.* "(*Amblyocentor*) *rhinocerinus* (Denny)" (page 171). Inconsistencies in the use of generic authorities, parentheses, references and synonymy, as well as misspellings, systematic lapses, and omissions (including no definitions of some technical terms—for example, opisthiosoma, alloscutum), cannot be illustrated here, but they reduce the volume's reliability and textual value for taxonomic reference. Failure to check the original reference for dating *Boophilus* from a "*nomen nudum*" (the date given, 1891, was actually the year of validation) exemplifies some other noncritical entries, while some of the pertinent Russian and Japanese literature has been overlooked.

The very cursory reference to disease relationships is also disappointing. Though the book understandably features taxonomy, the inadequately documented, often incomplete reference to the medical and veterinary importance of various species belies the introductory statements about the "immense medical and economic importance" of tick species under review; even the classic work of Smith and Kilbourne on the transmission of Texas fever in cattle by *Boophilus annulatus* is omitted. Nevertheless, the monograph will be useful in bringing together scattered systematic knowledge about the five genera treated. It is to be hoped that other specialists will be consulted during the draft stage of the next two volumes; this one obviously could have benefited from such consultation.

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Nutritional Evaluation of Food Processing. Robert S. Harris and Harry von Loesecke, Eds. Wiley, New York, 1960. xviii + 612 pp. Illus. $12.

In modern civilized societies more than 95 percent of the consumed food is processed to make it more palatable and nutritious and to keep it safe during storage, transportation, and distribution. The processing involves losses of essential nutrients which are affected by temperature, air, light, acidity, and solvents, or by a combination of these factors. This book evaluates these effects and establishes a scientific foundation for processing foods with a minimum loss of nutrients.

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New Books

Biological and Medical Sciences

Austin, C. R., Ed. *Sex Differentiation and Development.* Cambridge Univ. Press, New York, 1960. 208 pp. $8.50. This volume, Memoir No. 7 of the Society for Endocrinology, is the proceedings of a symposium held at the Royal Society of Medicine on 10–11 April 1958.

Bargmann, W., D. Peters, and C. Wolpers, Eds. *Proceedings of the Fourth International Conference on Electron Microscopy.* vol. 2, *Biologisch-Medizinischer Teil.* Springer, Berlin, 1960. 652 pp. DM. 196.

Brady, Roscoe O., and Donald B. Tower, Eds. *The Neurochemistry of Nucleotides and Amino Acids.* Wiley, New York, 1960. 304 pp. $10. Papers presented at a symposium that was held in 1958 to review the recent contributions in the field of nucleotide and amino acid chemistry in the light of application of these findings to the biochemical activities of the nervous system.

Brindley, G. S. *Physiology of the Retina and the Visual Pathway.* Arnold, London, 1960 (order from Williams and Wilkins, Baltimore, Md.). 309 pp. $7.50. Brindley, lecturer in physiology in the University of Cambridge, discusses how the human visual pathway works.

Cameron, Thomas W. M. Ed. *Evolution: Its Science and Doctrine.* Univ. of Toronto Press, Toronto 5, Canada, 1960. 253 pp. $5. Papers presented at a symposium held at the annual meeting of the Royal Society of Canada (1959) to commemorate the publication of *On the Origin of Species.*

Clark, F., and J. K. Grant, Eds. *The Biosynthesis and Secretion of Adrenocortical Sterioids.* Cambridge Univ. Press, New York, 1960. 119 pp. $5. Proceedings of the Biochemical Society Symposium No. 18, held in February 1959 and organized by J. K. Grant. The symposium was concerned with the production and secretion of steroid hormones by the adrenal cortex.

Clark, Marguerite. *Medicine Today: A Report on a Decade of Progress.* Funk and Wagnalls, New York, 1960. 360 pp. $4.95.

Cold Spring Harbor Symposia on Quantitative Biology. vol. 24, *Genetics and Twentieth Century Darwinism.* Biological Laboratories, Cold Spring Harbor, N.Y., 1959. 336 pp. $8.

Dawkin, M. J. R., and K. R. Rees. *A Biochemical Approach to Pathology.* Arnold, London; Williams and Wilkins, Baltimore, Md., 1959. 135 pp. $4.50.

Elliott, Alfred M., and Charles Ray, Jr. *Biology.* Appleton-Century-Crofts, New York, 1960. 732 pp. $8.

Gale, E. F. *Synthesis and Organisation in the Bacterial Cell.* Wiley, New York; Chapman and Hall, London, 1960. 117 pp. $3.50. The 1959 CIBA lecture in microbial biochemistry.

Garn, Stanley, M., Ed. *Readings on Race.* Thomas, Springfield, Ill., 1960. 289 pp. $6.75. Selected articles and papers from *American Anthropologist, Man, Human Biology, American Journal of Human Genetics,* and other sources, by B. Glass, C. Coon, W. Boyd, and others.

Reports

Isolation of an Unidentified Agent from the Respiratory Tract of Chickens

Abstract. A relatively stable filterable virus, not previously described, which hemagglutinated red blood cells of chickens, was isolated from the tracheae of birds suffering from an unusually severe outbreak of infectious laryngotracheitis. The latter disease concealed the infection caused by the new hemagglutinating agent, which was shown to cause, by itself, a mild and transient illness in susceptible chickens. Preliminary studies indicate that the new agent may be a new entity of the myxovirus group.

A respiratory disease tentatively diagnosed as infectious laryngotracheitis developed in a flock of 3-week-old chickens in southern California. Because the rate of spread and the mortality were unusually high, the case was referred to the University of California Agricultural Experiment Station laboratory at Davis for confirmation. The tracheae of affected chickens were moderately congested and contained petechial hemorrhages in the mucosa, and there were considerable amounts of caseous exudate in the larynx and in the lumen of the upper one-third of the trachea.

Chickens inoculated intranasally with antibiotic-treated or untreated suspensions of the tracheal exudates exhibited depression, dyspnea, rales, a serous nasal exudate, edema and congestion of the larynx, and hemorrhagic inflammation of the tracheae, beginning on the second day. The disease progressed rapidly and was followed by recovery or death by the 15th day. Chickens that recovered were immune to challenge with a virulent laryngotracheitis virus 2 to 6 weeks later and susceptible when challenged intramuscularly with a virulent culture of Newcastle disease virus. Sera of birds that recovered did not contain agglutinins for *Mycoplasma gallisepticum*, hemagglutinating-inhibition or neutralizing antibodies for Newcastle disease virus, or neutralizing antibodies for infectious bronchitis virus.

Tracheal exudates treated with antibiotics and inoculated on the chorioallantoic membrane did not kill embryos during a 6-day incubation period, but the membranes presented raised circular foci indistinguishable from those produced by laryngotracheitis virus; inoculation of chickens, by the intratracheal or intrabursal routes, with the infected membranes produced the typical disease reactions of laryngotracheitis. Inoculation of the tracheal exudates by the allantoic-sac route did not result in mortality in the first two passages, but the allantoic and amniotic fluids of the surviving embryos agglutinated chicken erythrocytes. The fluids of the second, third, and fourth passages hemagglutinated chicken erythrocytes in dilutions of 1:80, 1:160, and 1:320, respectively. A sporadic embryo mortality on the 4th or 5th day began with the third serial passage in embryos inoculated by either the allantoic-sac or chorio-allantoic-membrane routes. These preliminary studies indicated that the infectious exudates contained laryngotracheitis virus and a hemagglutinating agent that appeared to be serologically and immunologically distinct from Newcastle disease virus.

The allantoic and amniotic fluids of the third serial passage through embryos treated with chicken anti-Newcastle disease hyperimmune serum did not neutralize the hemagglutinating agent. The hemagglutinating agent could not be eliminated from the fluids of three additional serial passages, each of which was treated with known anti-Newcastle disease chicken sera. Chickens inoculated intramuscularly with the fluids of the last passage were asymptomatic for 15 days; sera collected from the chickens 3 weeks later inhibited hemagglutination of erythrocytes by the hemagglutinating agent but did not contain hemagglutinins or neutralizing antibodies for Newcastle disease virus. All of the birds were suspectible to challenge intramuscularly with a virulent strain of Newcastle disease virus.

Infected allantoic and amniotic fluids harvested from embryos inoculated in the first passage were treated with anti-Newcastle disease chicken serum, mixed with an equal volume of chicken anti-laryngotracheitis hyperimmune serum, incubated for 3 hours at 4°C, and inoculated into embryonating eggs on the chorioallantoic membrane. Hemagglutinins in the absence of lesions resembling laryngotracheitis, or death of embryos of the fourth, fifth, sixth, and seventh passages, were found in all embryos after a 5-day incubation period.

High mortality began with the eighth serial passage and increased to a proportion of seven of eight embryos inoculated in the tenth serial passage. A pool of the allantoic and amniotic fluids of the tenth passage was divided into aliquots and stored in sealed ampules for further study. Allantoic and amniotic fluids of the living embryos, as well as of embryos that died during the fourth through the tenth passages, consistently presented hemagglutinins and showed an infectivity titer of 5×10^8 per 0.2 ml of inoculum. Repeated infectivity and cross-neutralization tests in chickens and embryos proved that the laryngotracheitis virus was neutralized and eliminated from the infectious material.

The infectious agent in the allantoic and amniotic fluids of embryos of the tenth passage—fluids from which the laryngotracheitis virus was eliminated—repeatedly passed through the Seitz pad. Filtration of the fluids through graded gradacol membranes showed that the hemagglutinating agent passed through membranes with an aperture diameter of 235 $m\mu$ but not through membranes with an aperture of 140 $m\mu$; this range characterizes the myxovirus group. The hemagglutinating agent was not destroyed by heating infected allantoic and amniotic fluids at 56°C for 30, 60, 90, or 120 minutes.

Chickens 37 days old inoculated intramuscularly or intrabursally with the hemagglutinating agent showed no clinical symptoms during a 3-week observation period. Intratracheal inoculation produced mild rales, which occurred on the 4th to the 6th day in most of the chickens. The agent was readily recovered from the trachea 96 hours after inoculation. Sera of the chickens bled 25 days later contained hemagglutination-inhibition antibodies for the hemagglutinating agent but no inhibition anti-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

bodies for Newcastle disease virus. The birds showed susceptibility to a challenge dose of virulent Newcastle disease and laryngotracheitis viruses on the 28th day. Eight serial passages of lung suspensions of mice inoculated with the hemagglutinating agent by insufflation did not produce evidence of pneumonia or disease.

The hemagglutinating agent was readily propagated in monolayers of HeLa and pig kidney cells. A mild cytopathogenicity was detected by the 48th hour. Hemadsorption of guinea pig erythrocytes, as described below, was observed by the 48th hour without macroscopic changes in the cell sheets, but adsorption was more pronounced after the 144th hour of incubation.

Immune and hyperimmune sera (*1*) to related viruses were tested for neutralization of the hemagglutinating agent grown in embryonating eggs. Serial tenfold dilutions of the agent were mixed with an equal volume of a heat-inactivated (56°C for 30 minutes) serum diluted 1:10 in tryptose broth and held 1 hour at room temperature. Each mixture was inoculated into each of three 9-day-old embryonating chicken eggs, which were incubated at 35°C for a maximum of 6 days. Infectivity end points were determined by mortality after the 24th hour and the presence of hemagglutinins in the fluids of the surviving embryos. No neutralization of the hemagglutinating agent was demonstrated by antisera of influenza types A and B, mumps, fowl plague (strain Brescia and strain "N"), or Newcastle disease, laryngotracheitis, cello, or Crawley viruses. It was interesting to note that hyperimmune sera prepared with the fluids infected with the hemagglutinating agent in rabbits and chickens did not neutralize the homologous agent.

The hemadsorption test described by Vogel and Shelokov (*2*) and Shelokov *et al.* (*3*) was applied to tissue cultures of HeLa cells infected with the Montana strain of Newscastle disease virus and the hemagglutinating agent. After incubation at 37°C for 144 hours, 0.2 ml of a 0.4 percent guinea pig erythrocyte suspension was added to the washed infected cell sheet. Both agents hemadsorbed chicken, horse, bovine, and guinea pig erythrocytes. However, erythrocytes from guinea pig blood were adsorbed more heavily than those from the blood of the other species.

The hemadsorption-inhibition test showed that hyperimmune sera to the hemagglutinating agent and to Newcastle disease virus specifically inhibited hemadsorption of the homologous virus. None of the sera used in the chicken embryo neutralization tests described above inhibited hemadsorption of the new agent in tissue culture. In addition, parainfluenza 1 and 2 and infectious bronchitis virus immune sera failed to show evidence of inhibition of hemadsorption of guinea pig erythrocytes to the infected tissue culture cells (*3*).

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References and Notes

1. Antisera used in this study were obtained through the courtesy of several investigators as follows: influenza A and B and mumps, from Dr. E. Lennette, California Public Health Laboratory, Berkeley; fowl plague, strains Brescia and "N," from Dr. J. E. Williams, Animal Disease Eradication Division, Agricultural Research Service, Washington, D.C.; Crawley virus, from Dr. J. F. Crawley, Connaught Medical Research Laboratories, University of Toronto; *Mycoplasma gallisepticum*, from Dr. H. E. Adler, University of California, Davis; and cello, from Dr. V. J. Yates, University of Rhode Island, Kingston. Antisera of parainfluenza 1 and 2 were purchased from Microbiological Associates, Bethesda, Md. All other antisera were prepared by us.
2. J. Vogel and A. Shelokov, *Science* **126**, 358 (1957).
3. A. Shelokov, J. E. Vogel, L. Chi, *Proc. Soc. Exptl. Biol. Med.* **97**, 802 (1958).
4. This investigation was supported in part by grant E-445 of the National Institutes of Health, Bethesda, Maryland.

2 June 1960

Age Determination by X-ray Fluorescence Rubidium-Strontium Ratio Measurement in Lepidolite

Abstract. X-ray fluorescence analysis of several lepidolites whose rubidium and strontium concentrations had already been determined by neutron activation and stable isotope dilution, or both, indicates that this technique can be used for rapid nondestructive reconnaissance rubidium-strontium studies, and that an x-ray analysis method comparable in accuracy to isotope dilution can probably be developed for dating Precambrian lepidolites, as the simple technique presently used has many obvious possibilities for improvement.

Strontium in specimens of the mineral lepidolite has been shown to consist, in general, almost completely of the radiogenic isotope strontium-87, formed through the transmutation of radioactive rubidium-87 (*1*). Thus it is possible to determine the geological "ages" of lepidolites by determining total-Rb/total-Sr ratios alone, without isotopic analysis of the strontium.

Rubidium forms no minerals of its own, proxying for potassium in crystal structures; generally, the K/Rb ratio is in the range 200 to 300 (*2*). However, Rb is enriched in pegmatites, and K minerals in this environment may contain the element as a major constituent. The pink to purple lithium-bearing mica lepidolite, $([K,Rb]Li[Al(OH,F)_2]$-$Al(SiO_3)_3)$, can contain up to 4 percent Rb, and, in contrast, contains less than 500 parts of Sr per million, the amount depending on age. Hence the analytical problem for lepidolite age determination consists in accurately measuring Rb/Sr ratios ranging from about 90 to above 2500.

L. H. Ahrens *et al.* (*3*) attempted to determine lepidolite age ratios by optical spectrography, but were unable to achieve sufficient accuracy for Rb to make the method generally useful. Recently, however, several groups of workers (*4*, *5*) have achieved ratio determinations accurate to 5 percent or better by using the stable-isotope dilution technique, in which Rb and Sr concentrations are measured mass spectrometrically; and analysts at the Harwell installation have shown (*6*) that comparable accuracy can also be achieved by neutron activation analysis.

Stable isotope dilution analysis involves chemical and ion-exchange element separation, preparation of calibrated tracer isotope solutions, and the use of a mass spectrometer, while activation analysis requires the availability of a neutron source of high intensity, a radiochemical laboratory, and counting equipment. Both techniques are difficult and costly. Thus, there is a need for a rapid, inexpensive method of measuring Rb/Sr ratios, if only to make it easier to select samples worth studying by the other techniques.

X-ray fluorescence spectroscopy appeared to merit consideration for use in this application, because it is a rapid, nondestructive technique; hence, the present investigation was begun. A suite of samples whose Rb and Sr contents had previously been determined by isotope dilution and activation was available (*7*). Thus the Rb/Sr ratios determined for these "standards" by x-ray fluorescence could be compared against the ratios previously determined for them by the two established methods.

The x-ray spectrometer used was a General Electric XRD-5. Samples were bombarded by 50 kv CuK_α radiation at 50 ma. A LiF crystal followed by a 0.010 inch Soller slit was used for spectrum analysis. The detection system consisted of an argon-methane gas-flow proportional counter run at 1.825 kv, and a Berkeley scaler. Samples were ground to a fine flour in a small steel ball mill which had been precontaminated, and packed into aluminum holders for analysis. The samples were thick compared to the depth of pene-

Table 1. Comparison of data obtained by x-ray fluorescence and isotope dilution methods of Rb/Sr ratio determination in four lepidolites. Samples are from an intercalibration suite (*7*).

	L-12 Bikita	L-13 Ingersoll	L-14 Varutrask	L-15 Pala
		Isotope dilution data		
Age (million years)	2635 ± 100	1660 ± 70	1785 ± 50	107 ± 7
Rb/Sr (atoms)	92–100	145–157	144,144	2100–2600
Sr (weight ppm)	345 ± 15	80 ± 5	136 ± 10	6.9 ± 0.5
		X-ray fluorescence data		
Count/100 sec (av. for two runs):				
30° background	1153	1113	1146	1130
26.62° (Rb)	137,219	52,325	78,766	76,049
25.14° (Sr)	3064	1724	2010	1533
22° background	1174	1278	1205	1228
Derived net count rate:				
Rb count	136,050	51,155	77,595	74,880
error	±250	±160	±190	±185
Sr 298*	1417	365	562	94
Sr 238	1303	322	496	30
Sr 273a	1379 ± 52	352 ± 42	541 ± 45	74 ± 40
Sr 273b	1390 ± 65	320 ± 55	544 ± 58	69 ± 53
Rb/Sr 298	96	139	138	797
Rb/Sr 238	104	159	156	2490
Rb/Sr 273a	99 ± 4	146 ± 17	143 ± 12	1010†
Rb/Sr 273b	98 ± 5	159 ± 27	142 ± 16	1085‡

* 238, 273, and 298 are Rb/"Rb-tail" correction ratios; see text. † +1360, −385. ‡ +3585, −475.

tration of the incident radiation, and the total area of 14 mm by 20 mm was immersed in the beam, which was not homogeneous in density.

The first order $K\alpha_{1}\alpha_{2}$ doublets at 25.14° (strontium) and 26.62° (rubidium) were used for the analysis; actual background was measured at 30° and 22° and background values for the Sr and Rb angles were obtained by interpolation. In a single analysis, after peak-top identification, 100-second counts were made at each position, that is, 30-background, Rb, Sr, 22-background; then the process was repeated.

The first-order SrKα doublets lies, in lepidolites, in a "hanging valley" between the RbKα and Kβ peaks. Even for lepidolites very rich in strontium, such as the 2600-million-year-old Bikita specimen, the number of "Rb-tail" counts falling at the Sr position is an appreciable fraction of the Sr count rate, and the background count rate is of the same order of magnitude. The situation for a young sample, such as the approximately 100-million-year-old Pala lepidolite, appeared at first sight to be hopeless, as the expected Sr count rate is less than one-tenth the background and overlap count rate total. The possibility of using the second or third order lines, where Rb and Sr are better separated, was investigated but given up, both because of the overlap of Sr and Fe peaks, and because of the great attenuation of the Sr count. Rb-Sr overlap at the first order was reduced somewhat by replacing the original 0.020 inch Soller slits with 0.010 inch slits.

The following procedure was adopted for computing Rb/Sr ratios: (i) It was assumed that equal numbers of Rb and Sr atoms give equal numbers of counts at the detector; this assumption is expected to be erroneous by only a few percent. (ii) Identical 25.14° and 26.62° background corrections were used for all samples. These were based on eight 100-second counts each, at 22° and 30°. If matrix effects are present, such averaging will introduce error; but the results obtained show already that any such effect must be quite small within this group of samples. Background correction numbers for the Sr and Rb angles were obtained from the data taken at 22° and 30° by straight-line interpolation. As the line may be slightly curved, some bias may have entered here. (iii) Rb was determined by subtracting the assumed 26.62° background from the total count. The Rb to Rb-background count ratio was always high (>50). (iv) Sr was computed by subtracting both the background contribution and the computed contribution of "Rb-tail" counts to the total count at 25.14°.

A "Rb-tail" correction was computed with the isotope dilution Rb/Sr ratios and the observed x-ray fluorescence Rb count rates to calculate the number of Sr counts expected both for the Bikita and Pala samples, and subtracting each number, plus interpolated background, from the total 25.14° counts. For Bikita, the "Rb-tail"/Rb ratio thereby obtained was 1/298, while for Pala it was 1/238.

A direct measurement of the overlap in a Sr-free RbCl sample gave an intermediate ratio, 1/273. Since no decision is yet possible as to which method is preferable, four sets of Sr-concentration data are given in Table 1. The first three are based on the three determinations of Rb-tail/Rb ratio just described. Thus, in the data on strontium labelled "Sr 298" the Bikita sample was used as the sole standard and the other three specimens as unknowns; in the "Sr 238" set the Pala sample alone was used as a standard, Bikita becoming an unknown; and for the "Sr 273a" line the tail correction comes from the RbCl experiment so that all the lepidolites are unknowns. In computing the "Sr 273b" line the tail ratio 1/273 was also used, but for the background at 25.14°, values based on the 22° and 30° data for each particular sample were used.

The isotope dilution and neutron activation data for these samples are also summarized in Table 1. Only analysis ranges are given; further details are given in the paper already referred to (*7*). In summary, the samples have been analyzed by from two to four groups by isotope dilution and by one group by neutron activation, with the agreement shown. All of the x-ray fluorescence data are in sufficiently good agreement with the isotope-dilution determined ratios to be useful for reconnaissance analysis purposes, and for the three ancient lepidolites, the x-ray fluorescence ratios appear to approach the isotope dilution analyses in quality.

X-ray fluorescence analysis provides an inexpensive, rapid, and reasonably precise method of reconnaissance Rb/Sr ratio and concentration determination; the present technique has a threshold of about 5 ppm for these elements. It appears it may be possible to develop the technique sufficiently to make it feasible to determine Rb/Sr ages by x-ray fluorescence analysis alone (at least for lepidolites of Precambrian age), with an accuracy comparable to that achieved by stable isotope dilution, but with a much smaller expenditure of time and money. The technique also has, of course, the advantage of being nondestructive (*8*).

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press) and also, by individual analysts, in (*3*) and (*4*) above. The samples used are: PSU sample L-12 = LN45, Bikita Quarry, Southern Rhodesia; L-13 = L-A106, Bob Ingersoll Mine, South Dakota; L-14 = L-A110, Varutrask Quarry, Sweden; and L-15 = L-A107, Pala Mine, San Diego County, Calif.

8. This work was supported by the Mineral Conservation section of Pennsylvania State University. I wish to thank Dr. Egon T. Degens, Mr. Dieterich Welte, Mr. Werner Deuser, and Mr. Robert Shutt for instruction and assistance. Samples were graciously provided by Prof. Harrison Brown of California Institute of Technology (the M.I.T. supply being exhausted). This report is contribution No. 60-2, College of Mineral Industries, Pennsylvania State University.

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Rates of Submergence of Coastal New England and Acadia

Abstract. Altitudinal and carbon-14 age determinations of in-place *Pinus strobus* stumps of drowned forests at Odiorne Point, N.H., and Grand Pré and Fort Lawrence, Nova Scotia, yield apparent average rates of submergence of 3.1, 14.5, and 20.3 feet per 1000 C^{14} years, respectively. Rate differences are assessed in terms of eustatic rise of sea level, crustal movements, and tidal effects.

Radiocarbon dates for drowned trees along the New England coast have been reported by several investigators (*1–4*), but these determinations only fix the general period of submergence of the coastline. On the basis of earlier work on drowned forests (*5*), one of us (C.J.L.) was able to plan the present study, which provides a measure of the rate of coastal submergence at each of three drowned-forest sites. The initial assumptions of this study were that rising salt water killed the trees from which wood could be dated by C^{14}, that submergence of the forests was progressive at each site, and that the approximate average rates of submergence could be determined from a knowledge of the dates of death of trees at sufficiently different altitudes at each locality.

The drowned forest at Odiorne Point, N.H., near lat. 43°2′ N., long. 70°44′ W., consists of the remains of a coniferous stand whose stumps and fallen trunks are found within a protected cove measuring about 500 feet in diameter. The stumps are rooted in a firm woodland peat, ranging from 2 to 4 feet in thickness, that overlies till and bedrock.

The famous drowned forest at Fort Lawrence, N.S., near lat. 45°50′ N., long. 64°17′ W., has now lost most of its exposed soil and the stumps rooted in it. The site can be reached by following Dawson's directions (*6*). Samples of the forest soil, described by Dawson as "black vegetable mould, resting on a white, sandy subsoil" (*6*, p. 441), proved to be clay loam colored with forest humus to a depth of 6 inches. The "white" layer is underlain by 8 to 9 inches of red sandy loam, which in turn rests on till.

At Grand Pré, N.S., the remains of a forest extend over at least 100 acres of mud flats exposed at low tide on the north side of Boot Island (lat. 45°8′ N., long. 64°17′ W.), near the south shore of the Minas Basin. Stumps are rooted in 15 to 18 inches of gray clay soil that rests upon a pale-red clay layer varying in thickness from 12 to 24 inches, underlain by stony till.

Samples for C^{14} dating were cut from stumps of four white pine (*Pinus strobus*) trees at each of the three sites. The altitudes of the bases of the stumps were determined by standard leveling techniques. Stump altitudes were related to mean sea level, as determined from tide-table data and the observed high-tide mark for the date of the field work at each site. The precise altitudes, relative to mean sea level, of the dated stumps are not critical to the determinations of rates of submergence; only the relative differences in altitudes between stumps at each site are critical. These last altitude values are believed accurate to ± 0.4 foot.

The wood used for C^{14}-age determinations reported in the present paper was taken from the outer rings of the highest and lowest stumps at each site. It was free from contamination by borers and other organisms. Stump-base altitudes and C^{14} ages are shown in Fig. 1. The following speculations about the data are offered.

1) If the rates of coastal submergence were constant at each of the three sites between 4450 and 3200 years before the present (B.P.), the average rate of submergence at Odiorne Point corresponds closely to the approximate average rate of postglacial rise of sea level (Fig. 1), as determined from data presented by Shepard and Suess (*7*, Fig. 1). Crustal stability at Odiorne Point during this time interval would thus be implied, but later upwarping would be required, because the stumps are some 12 feet closer to present sea level than the materials that were dated in Shepard and Suess's study (Fig. 1). Also, the apparent average rate of submergence in the Fort Lawrence–Grand Pré area would amount to roughly three times that which would have been induced by the rate of postglacial rise of sea level determined from Shepard and Suess's data. The greater rate of submergence could probably best be explained as resulting from slow crustal downwarping in the Nova Scotian region, simultaneous with sea-level rise. The possibility that the Nova Scotian submergence rate reflects wholesale tree death during part of the time interval, and is attributable to the breaking of barriers between ocean and forests or to a rapid displacement of the crust, is improbable (*5*, p. 614). Relatively long periods of time were involved in the killing of successively higher trees by the rising salt water (*5*). Another consideration is that greater submergence rate might in some way be related to a progressive increase in tidal range, as continued submergence brought ocean waters farther and farther into the Fundy embayment (*8*, p. 580).

2) If submergence at each site coincided with eustatic oscillations of sea level of the type suggested by Fair-

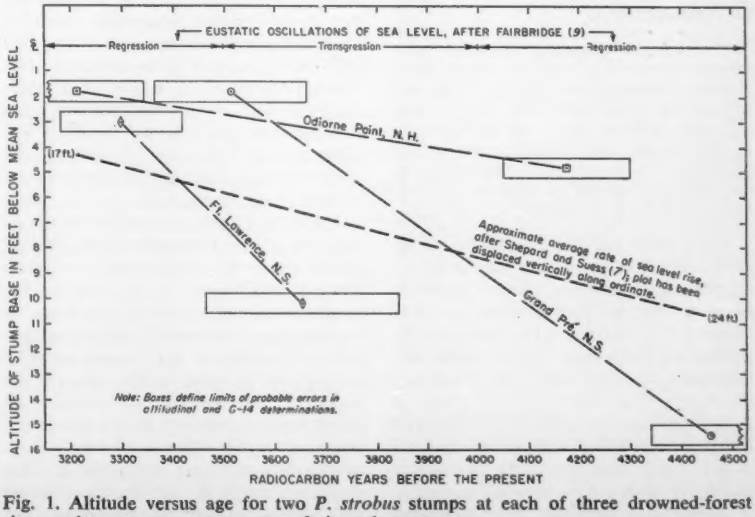

Fig. 1. Altitude versus age for two *P. strobus* stumps at each of three drowned-forest sites, and apparent average rates of site submergence.

bridge (*9*, Fig. 1), the submergence sequences may be considerably more complicated than our Fig. 1 indicates. Because three of our trees were killed by rising salt water during two of Fairbridge's postulated regressions of sea level [see also Hussey (*4*, Table 1), 3250 ± 200 B.P.], their deaths would have to be explained by crustal downwarping at rates in excess of the eustatic regressions.

We wish to withhold judgment on these speculations, pending C^{14} dating of six samples from stumps at intermediate altitudes at each of the three sites (*10*).

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Antithyroid Effects of Aminotriazole

Abstract. Aminotriazole, like other antithyroid compounds and low-iodine diets, produces adenomatous changes in the thyroid glands of rats when fed continuously for long periods. Such changes are reversible if the antithyroid regime is discontinued. The antithyroid compounds that are naturally present in food are not regarded as causes of cancer.

The action of the Food and Drug Administration in seizing shipments of cranberries containing alleged residues of the herbicide aminotriazole (3-amino-1,2,4-triazole) (*1*) makes it desirable to summarize briefly some unpublished findings with this substance (*2*, *3*).

A large number of compounds have the property of inhibiting the thyroid gland from reacting with inorganic iodide to form the thyroid hormone (*4*). Prominent among these "antithyroid" compounds are certain substances containing the –NH–CS– grouping, including thiouracil, and a number of aminoheterocyclic compounds, including aminotriazole (*5*). Many foods, including cabbage, turnips, peas, beans, strawberries, and milk, contain antithyroid substances (*6*), one of which, L-5-vinyl-2-thiooxazolidone ("goitrin"), has been isolated in quantities as high as 200 parts per million from rutabagas and has a potency equal to that of thiouracil as measured in human subjects (*7*).

In experiments in our laboratories (*3*) enlargement of the thyroid gland in rats and pronounced lowering of uptake of administered radioiodine by this gland were produced by feeding diets containing 60 or 120 parts per million of aminotriazole for 2 weeks. There were no significant changes in the gland at levels of 15 and 30 parts per million.

In studies carried out at a consulting laboratory (*8*), rats were fed diets containing 0 (group 1), 10 (group 2), 50 (group 3), and 100 parts per million (group 4) of aminotriazole. After 24 months the animals were sacrificed and a histological examination was made of some of the thyroid glands. No tumors were found in five thyroid glands examined in group 1, although there was one cystic follicle with papillary change that is typically the forerunner of a very large cystic adenoma. One adenoma was found in ten thyroid glands in group 2. Two of the 15 glands in group 3 and 17 of the 26 glands in group 4 were found to be adenomatous. One gland in group 3 and four in group 4 showed changes which were interpreted by some pathologists to be adenocarcinomatous and by others to be nonmalignant. The exact significance of these changes in relation to spontaneous thyroid pathology in rats of this age needs extended discussion beyond the scope of this brief article and will be reported elsewhere (*2*). After 17 weeks, aminotriazole was withdrawn from the diet of a fifth group of rats that had received 500 parts per million, and 2 weeks later their thyroid glands were found to be normal in appearance.

It has long been recognized that suppression of the function of the thyroid gland in rats by administration of antithyroid compounds or by deprivation of dietary iodine leads to an increase in the production of the thyroid-stimulating hormone of the pituitary and a consequent increase in the size of the thyroid gland (*9*). The prolongation of either type of regimen led to the emergence of thyroid adenomata (*9*, *10*, *11*) which were not seen when the feeding of the antithyroid diet was alternated with periods of control diet (*10*).

By continuing the feeding of comparatively high levels of such antithyroid substances as thiourea and thiouracil to mice, lung nodules of thyroid-like tissue were eventually produced (*12*). Adenocarcinomatous tumors were produced in rats by prolonged treatment with thiourea. No metastases were found and the authors regarded this absence as evidence against any direct carcinogenic effect of thiourea, but rather as reflecting the overproduction of a normal body constituent, thyroid-stimulating hormone (*13*).

These and other observations make it apparent that an antithyroid regimen in experimental rats and mice can be predicted to result eventually in the appearance of thyroid tumors. Some of these may reach a stage that is regarded by some authorities as adenocarcinomatous, although this point is debated by other investigators who consider the changes as resembling benign tumors (*14*). In any case, such a stage has been described only if the treatment corresponded to complete blocking of the thyroid gland taking place continuously for most of the normal lifetime of the animal. Moderate or intermittent treatments with antithyroid substances have not been reported to lead to the production of tumors. Indeed, the consumption of foods that inherently contain antithyroid substances is not usually regarded as a potential cause of cancer in human beings. Furthermore, thiouracil and its congeners are commonly used clinically in the treatment of hyperthyroidism, thus giving additional clinical experience in the use of compounds of this type (*15*).

Recently Hoshino has reported in a preliminary communication that injections of aminotriazole significantly delayed the production of liver cancer in rats that received the carcinogenic dye, 4-dimethylaminoazobenzene (*16*). This recalls the finding by Paschkis and coworkers (*17*) that another antithyroid compound, thiouracil, protected the livers of rats against the carcinogenic action of 2-acetaminofluorene.

The characterization of the antithyroid compound aminotriazole as a cancer-producing substance would seem to be questionable and in any case needs careful definition of conditions. Foods containing antithyroid substances are universally present in ordinary diets.

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18 April 1960

Discrimination of Tones during Reinforcing Brain Stimulation

Abstract. Hungry animals were trained to press a lever for brain stimulation. Different tones were presented concurrently with the stimulation. A second lever delivered food only during critical tone periods. Animals were able to discriminate tones presented concurrently with rewarding intracranial stimulation, and they also interrupted self-stimulation behavior to respond appropriately under other reinforcements.

Since the report by Olds and Milner (*1*) that rats will perform tasks if rewarded by electrical stimulation of specific subcortical brain areas, a number of studies have demonstrated the phenomenon with guinea pigs (*2*), cats (*3, 4*), and monkeys (*5, 6*). The phenomenon may be referred to as self-stimulation when the animal is permitted to determine the rate of intracranial stimulation by its rate of responding. Previous studies have reported the distribution of rewarding sites within the central nervous system (*7*), the effect of various schedules of intermittent reinforcement on self-stimulation behavior (*8, 9*), the rate of extinction following the withholding of intracranial stimulation (*8, 10*), and the effect of various states of deprivation such as food and sex deprivation (3, *11*).

Little information is available, however, concerning the ability of an animal to respond to stimuli during intracranial stimulation. There is evidence that animals are unresponsive to environmental cues while receiving rewarding brain stimulation. In conditioned emotional response experiments, for example, animals stop working for food if a clicking sound which had previously been associated with a painful shock is presented (*12*), but Brady (*5*) has recently demonstrated that when rewarding brain stimulation is substituted for food, animals continue responding during the period of clicker presentation. Are the animals capable of sensing and interpreting stimuli presented concurrently with intracranial stimulation? The experiment reported here was designed to answer this question. The results also shed some light on the ability of animals to interrupt self-stimulation to respond under other kinds of reinforcement.

Seven male albino rats, weighing approximately 300 gm each at the beginning of the experiment, served as the subjects. Bipolar electrodes were implanted stereotaxically in the middle and posterior hypothalamus. Postoperatively, the rats were deprived of food until their weight was reduced to 80 percent of their weight on an ad libitum feeding regimen, and then were maintained at this weight. They were then trained to press a lever to receive a 0.2 ml cup of diluted condensed milk.

Tone discrimination training consisted of presenting 1.5-minute periods of tone A (1000 cy/sec) and tone B (200 cy/sec) in a random sequence. Responses were rewarded with milk only during tone A periods. When more than 80 percent of the responses occurred during the tone A periods, the subjects were trained to press a second lever for brain stimulation. The electrical stimulus consisted of a 0.5-second train of paired biphasic square waves presented at a frequency of 100 presentations per second. The pulse pairs were 0.2 msec in duration, and they were separated by an interval of the same duration. Responsiveness to the intracranial stimulation varied, and an intensity was selected which yielded stable lever-pressing rates.

After stable response rates for brain stimulation were established, 1.5-minute periods of either tone A or tone B were randomly presented with an average interval of 3 minutes between periods. During each tone period, every response on the brain-stimulation lever produced the appropriate tone. The onset of the tone followed the onset of the brain stimulus by 15 msec and ended 30 msec before the termination of the stimulus. If the animal switched to the food lever during a tone period, the tone started beeping independently of lever presses for the balance of the 1.5 minutes. The ending of the beeping tone served as a signal to the animal that food was no longer available. Thus, until the animal had switched to the food lever, tones were presented only concurrently with brain stimulation. Pressing the food lever was rewarded only during the tone A periods, while brain stimulation followed all responses on the brain-stimulation lever. Each test contained nine tone A and nine tone B periods.

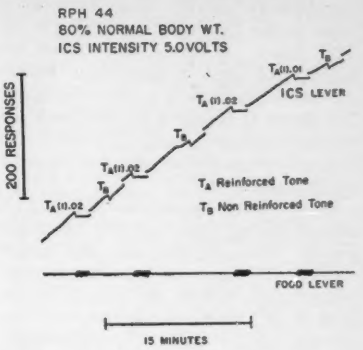

Fig. 1. Typical record illustrating responses on the intracranial stimulation (ICS) and food levers.

There is clear evidence that the rats were able to discriminate between tones presented concurrently with brain stimulation. Figure 1 shows the activity of a typical rat on the brain-stimulation and food levers after eight testing sessions. Pressing of the food lever is indicated on the horizontal, while activity on the brain-stimulation lever is cumulated in the upper curve. The pen recording brain-stimulation-lever activity was deflected downward during the tone periods; these can be identified from the labels. It can be seen that the animals switched to the food lever only during tone A periods and returned

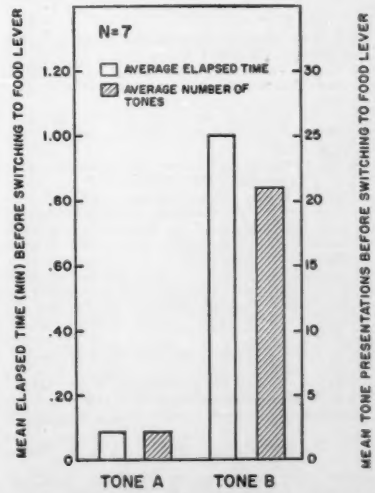

Fig. 2. Bar graph illustrating mean elapsed time and mean number of tone presentations before the switch to the food lever.

immediately to the brain-stimulation lever at the termination of this period. The figure in parenthesis gives the number of tone presentations before the animal switched to the food lever; this figure is followed by the time (in minutes) which elapsed between the first tone presentation and the response on the food lever.

Figure 2 summarizes the data for all animals during their last four tests. Bar graphs were plotted for the mean elapsed time and for the mean tone presentations before the switch to the food lever. The animals required an average of 2.1 tone A presentations, with an average elapsed time of 0.07 minutes, before switching to the food lever, while they rarely switched to the food lever during the tone B period. For purposes of calculation, 1.5 minutes was scored when the animals did not respond on the food lever during a tone period.

The results demonstrate not only that the animals were capable of distinguishing between the two tones during intracranial stimulation, but that, at least under certain conditions, they were capable of terminating self-stimulation to respond to other reinforcements. In subsequent experiments animals were found to differentiate between stimuli appearing simultaneously with brain stimulation, but whether or not they responded depended upon the reinforcing effect of the brain stimulation and the consequence of responding to the second lever.

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6 May 1960

Allelic Genes in the Housefly Producing Modified Enzymes That Cause Organophosphate Resistance

Abstract. In all of six phosphate-resistant strains of *Musca domestica* L. a mutant gene is present which produces an altered ali-esterase. The modified enzymes are no longer irreversibly inhibited by the oxygen analogs of the insecticides to which the strains are resistant but can slowly convert them. In five of the strains the resistance is caused by this gene only.

It is generally conceded that the fundamental effect of gene mutation is the production of altered proteins. However, in only a few cases has this actually been demonstrated (*1*). From the study presented here, it appears that organophosphate resistance in the housefly, *Musca domestica* L., is mainly due to the production of such an altered protein. The adaptive value of this change is of great importance to the species under the altered conditions of life.

In the housefly the use of the organophosphate insecticides Parathion, Diazinon, and Malathion (*2*) has led to the development of resistant strains (*3*). Since these phosphorothionates, after conversion to the corresponding oxygen analogs paraoxon, diazoxon, and malaoxon, act as strong inhibitors of different esterases in the insect, a comparison was made between the esterase activities shown by homogenates of resistant and susceptible strains. A curious difference was found: homogenates of all the phosphate-resistant strains studied showed a very low ali-esterase activity toward substrates such as methyl- and phenylbutyrate, whereas high activity was found in all the susceptible strains investigated (*4*). Low activity has now been observed in five strains resistant to Parathion and Diazinon from Europe and North America and in two strains from North America resistant to Malathion. High activity is present in six organophosphate-susceptible strains, some of which are resistant to the chlorinated hydrocarbons DDT and γ-BHC (benzene hexachloride).

The low esterase activity was found to be caused by an autosomal gene (*5*) (indicated as *a*; the corresponding gene for high esterase activity is indicated as a^+). The mean esterase activities toward methylbutyrate of a homogenate of single male flies of the genetic constitutions *aa*, aa^+, and a^+a^+, respectively, are about 50, 175, and 300 (expressed as microliters of CO_2 produced in the Warburg manometer in 30 minutes under certain conditions). Furthermore, it was found that in four out of five strains studied the phosphate resistance depends mainly on one gene (*5*, *6*), which is identical with the *a* gene. In the fifth strain the *a* factor is responsible for only a part of the resistance; in addition, one or more other resistance genes are present which do not affect esterase activity.

Although the *a* genes seem to be equal in their effect on the esterase activity, they differ widely in the specificity and degree of resistance which they confer on the strains. At least three different *a* genes have been discerned so far (Table 1, strains D, C, and G). The fact that they have the same influence on the esterase activity and all cause resistance suggests that they may be alleles. Nguy and Busvine (*6*) studied this possibility for the Parathion-Diazinon resistant strain C and the Malathion resistant strain H. Hybrids of these strains were backcrossed with the susceptible strain. Since the offspring consisted of about 50 percent Malathion resistant and 50 percent Parathion-Diazinon resistant flies (resistance is semidominant), these workers concluded that the two genes are allelic or otherwise closely linked.

We studied the possible allelism in the Parathion-Diazinon resistant strains F and C and the Malathion resistant strain G, estimating the esterase activity instead of resistance. The crosses F × G and C × G were made, and f_1 and f_2 generations were obtained. The f_1 flies had low esterase activity. Low

Table 1. Susceptibility and in vitro breakdown capacity in some strains of houseflies. The in vitro breakdown was measured in the following way. To a series of samples of homogenates, increasing amounts of inhibitor were added. After 2 hours of incubation the presence or absence of inhibitor was tested by adding fresh cholinesterase. The breakdown capacity was calculated by taking the mean of the amount of the inhibitor that had been converted completely and the next higher amount which had not yet been completely degraded. In strains without a breakdown enzyme a certain amount of inhibitor was bound by the homogenate. However, this amount (indicated by an asterisk) does not increase with time.

LD_{50} Diazinon (μg/jar)	Breakdown diazoxon (mμg/2 hr)	LD_{50} Malathion (μg/fly)	Breakdown malaoxon (mμg/2 hr)
	Strain S		
2	5*	0.6	4*
	Strain D		
12	15		
	Strain C		
62	72	2.5	13
	Strain G		
4	7*	25	130
	Strain F		
100	24		
	Strain F_a		
11	17		
	Strain F_b		
11	5*		

activity was also found in all of the 98 individual flies tested of the f_2 of the cross F × G and in all of the 52 flies tested of the f_2 of C × G. If two nonallelic genes for low esterase activity had been present, ¼ of the f_2 flies would have had only one *a* gene (intermediate esterase activity) and 1/16 would have had no *a* genes (high esterase activity). Since low activity was found, the above data indicate allelism or close linkage.

It has been found that homogenates of flies resistant to Parathion and Diazinon contain an enzyme which can degrade the oxygen analogs of these insecticides (*7*). From the fact that the activity of this enzyme parallels the degree of resistance in two of the strains in which resistance is caused by one gene only, we concluded that the enzyme is made under the influence of the *a* gene (Table 1, strains D and C). The breakdown enzymes in the resistant strains thus would be modifications of the ali-esterase normally made under the influence of the a^+ gene. Two sets of data provide evidence to support this conclusion.

First, a substrain F_a was obtained in which the *a* factor of the resistant strain F was incorporated into the genome of the susceptible strain S. This was done by crossing strain F with strain S and by subsequent repeated backcrossing to strain S. Concurrently, selection for low ali-esterase activity was made. Homogenates of the F_a flies showed a breakdown capacity which was only a little lower than that of the F flies. This shows that breakdown capacity and low ali-esterase activity are dependent on one and the same gene. This was further borne out by the absence of breakdown in substrain F_b (Table 1). This strain showed about the same resistance as strain F_a but contains the a^+ allele, and its resistance must be brought about by one or more other factors (strain F also has more than one resistance factor). In a similar experiment attempts were made to transfer the *a* gene from the highly resistant strain G into strain S. This strain should have shown high resistance and breakdown capacity. However, it proved impossible to obtain such a strain in this case, probably as a result of the presence of a lethal factor (*5*).

Second, since the *a* genes are alleles and in strains resistant to Parathion and Diazinon a breakdown enzyme is produced for the corresponding phosphates, Malathion resistant strains should produce an enzyme capable of degrading malaoxon. This was found to be the case (*7*).

The mutant enzymes present in the strains resistant to Parathion and Diazinon have been shown to possess a very high affinity for the oxygen analogs of these organophosphates. Nearly complete saturation of the enzyme is obtained with substrate concentrations of the order of $10^{-5}M$. Since it has been found that the ali-esterase is readily phosphorylated by these organophosphates (*8*), the main difference is in the rate of dephosphorylation. This is practically zero for the ali-esterase, and thus there is an irreversible inhibition, while a slow but definite turnover is present in the breakdown enzymes. If it is assumed that the concentration of the "mutant" enzymes is equal to that of the ali-esterase, the turnover number must be of the order of 0.1 per minute.

Although most of the activity of "resistant homogenates" toward substrates such as methylbutyrate is brought about by other enzymes, there is evidence that a small part of the activity is due to the breakdown enzyme. This evidence was obtained by comparing the hydrolytic activities of homogenates in the presence of the organophosphates with the activities of other homogenates in which the added phosphate had been completely degraded. Though the difference was small, the activities of the latter homogenates were significantly higher. This may well be explained by the assumption that the breakdown enzyme, which will be saturated by the organophosphates as long as these are present, regains its reactivity to methylbutyrate after the degradation of the organophosphates has been completed. This activity is only 1 to 3 percent of the activity of the ali-esterase of the susceptible flies, but, still, this finding supports the view that ali-esterase and breakdown enzymes are related.

It can be calculated that the turnover number of the breakdown enzyme for methylbutyrate is at least 10^4 times that for the organophosphates. Yet there seems little doubt that the degradation of the organophosphates constitutes a physiologically very important feature of the breakdown enzyme.

The natural function, if any, of the ali-esterase in the flies is unknown. It is possible that the breakdown enzymes are as active as the ali-esterase in converting some unknown natural substrate. Whatever the function may be, evidently it can be dispensed with for at least some hours, since the enzyme is blocked as long as it is protecting the insect from intoxication by degrading the organophosphate.

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11 February 1960

New Test for the Biological Assay of Oxytocin

Abstract. A strip of mammary gland is removed from a lactating rabbit and suspended in a bath. The contractions of the strip are recorded isometrically. The strip shows no spontaneous activity and responds with reasonable linearity and stability to oxytocin at concentrations ranging from 0.5 to 10 milliunits per milliliter.

Lactating rabbits from the 15th to the 30th day postpartum are anesthetized with a short-acting barbiturate. A mammary gland is separated from skin and abdominal fascia and a radial strip of gland tissue removed, two parallel cuts being made from the periphery to the teat. Strips so obtained are usually 3 to 4 cm long, 0.5 cm wide, and 2 to 4 mm thick. The strip is suspended in a bath of small volume (1.5 ml) containing Tyrode's solution at 38°C. Isometric contractions of the strip are recorded with a strain gauge of high sensitivity (Statham Model, G7A ± 0.15 oz) and a recording galvanometer

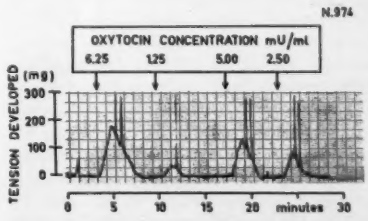

Fig. 1. Isometric recording of the contractions of an isolated strip of rabbit mammary gland. There are no spontaneous contractions. Concentrations of oxytocin from 1.25 to 6.25 milliunits (mU) per milliliter evoke responses which are seen as slow increases in tension, recovery from which takes several minutes. The brief spikelike effects are due to changing the bath fluid.

system (Sanborn Poly Viso). If kept at 3°C in Tyrode's solution, such strips retain for several days their ability to react to oxytocin.

The strips of mammary gland show no spontaneous contractions. They respond consistently to oxytocin, developing tensions which may reach values of the order of 500 mg (Fig. 1). Contraction develops slowly, taking about 1 minute from the onset to the peak. Relaxation occurs even if oxytocin is not washed out from the bath. The lowest concentration of oxytocin detected by the mammary gland strip has been 0.1 milliunit/ml. Within a range from 0.5 milliunit/ml to 10 milliunit/ml the tension developed by the contraction of the strip is in direct linear relationship with the concentration of oxytocin (Fig. 2). The dose-response curve remains remarkably constant for several hours if the resting tension is readjusted to a constant value before each observation. The most commonly used values of resting tension are between 50 and 100 mg. Furthermore, the mammary gland strip does not contract when heparinized blood or plasma is added to the bath, thus making possible the direct determination of oxytocin in these fluids.

The linearity and the stability of the dose-response curve of the mammary strip offer considerable advantages over other tests currently employed for the assay of oxytocin. Moreover, with strips of mammary tissue, as with the mammary gland studied in vivo, specificity is high and spontaneous activity absent. Because of these properties the mammary strip compares favorably with the isolated rat uterus which frequently exhibits spontaneous activity and also responds to a great variety of substances occurring naturally in normal blood.

The sensitivity of the mammary strip test is 5 to 10 times greater than that of the response of the intact mammary gland to oxytocin given intravenously. Sensitivity is, however, less than that obtained in vivo when the oxytocin is injected into the arteries supplying the mammary gland (*1, 2*). It is also less than the sensitivity of the superfused rat uterus (*1, 3, 4*).

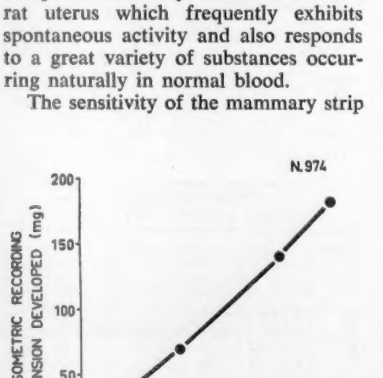

Fig. 2. The responses illustrated in Fig. 1 are plotted to show the linear relationship between concentration of oxytocin and tension recorded from the isolated strip of mammary tissue.

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8 February 1960

Feather Mites and Ornithosis

Abstract. Ornithosis virus has been isolated from several species of poultry ectoparasites, suggesting for the first time that this too may be a vector-borne infection.

A virus of the ornithosis group, for which we prefer the designation *Bedsonia* rather than *Miyagawanella*, has been isolated by mouse passage from ectoparasites collected under two epizootiologically different circumstances in two widely separated geographical areas.

In the first case there had been some serologic, but no clinical, evidence of this infection in a chicken flock in the preceding 3 years. When an observer reported an extensive infestation with mites, the Hooper Foundation requested that some of these be collected so that they could be examined for the ornithosis virus. The ectoparasites were collected from a rooster with an indirect complement fixation titer of 1:16, and in the first intraperitoneal mouse passage and in the subsequent ones there was gross and microscopic evidence of the virus. These insects were not identified, but in the further pursuit of this very interesting observation, lice, some identified by an entomologist to be *Menopon gallinae (M. pallidum)* were collected from ten hens and again the virus was isolated.

Although it has been known since early in the 1930's that activation of latent infection accounts for the sporadicity of this infection, in some cases this explanation has not sufficed, in incubator-hatched poultry for example. This isolation suggested a hitherto unrecognized virus-perpetuating system. But the misleading interference of masked infections in laboratory mice, although there was no special reason to suspect them in this observation, and the fact that the isolation was made in a laboratory where other work on this virus is being carried on, made it necessary to make some further studies.

Since ornithosis had been occurring annually and inexplicably in a turkey flock, the next step was to see whether there might be infected ectoparasites on the premises of that flock. A public health official who had been participating in investigations of the flock collected miscellaneous material, including insects, from nests in which there had been no turkeys for about 2½ months. Most of the insects were still alive when they were identified and separated for the isolation tests. Mouse passage of 117 pools has again revealed the virus, despite the fact that the mites could not have fed on infected turkeys for at least 3½ months before the test. Two of the infected pools consisted of Glycyphagidae, Berlese, probably *Glycyphagus domesticus*. The other was a mixture of *Haemogamasus, Haemolaelaps, Ornithonyssus*, and *Cheyletus*. Isolations have been made also from *Cheyletus* and a *Mesostigmata*, possibly Arctacaridae.

This information is given to stimulate others to investigate this possibility further and to consider such ectoparasites as potential reservoirs or vectors of other infections.

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9 May 1960

Rapid and Reversible Block of Electrical Activity by Powerful Marine Biotoxins

Abstract. Puffer-fish poison and clam poison reversibly inhibit conduction in single nerve fiber preparation of frog in a concentration of $3 \times 10^{-10} M$. In the isolated electroplax of *Electrophorus electricus* higher concentrations block both transmission and conduction. Neither toxin is a potent acetylcholinesterase inhibitor. The mechanism of action of these toxins in blocking transmission and conduction has not yet been established.

Extremely toxic compounds have been isolated from certain marine animals, fish and invertebrates. Some of these toxins have been isolated and purified. Their molecular weights are

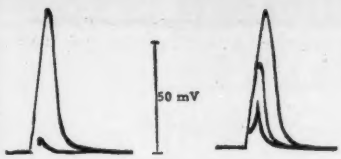

Fig. 1. Effect of clam poison on the mononodal action potential of a single nerve fiber preparation of the frog sciatic nerve (*Rana pipiens*). At $3 \times 10^{-10}M$ concentration (10^{-4} μg toxin per milliliter of solution), this toxin blocks conduction in 30 seconds. After return to Ringer's solution, conduction was restored within 1 minute (*p*H, 7.7; temperature, 23°C).

known but not their structure. Knowledge of the mode of action of these toxins is rather limited, but it is known that some of them block conduction and neuromuscular transmission reversibly (*1*). On the suggestion of B. Jandorf we have tested, on the electrical activity of conducting membranes, the effects of two of the toxins (*2*): the clam poison, prepared according to the method of E. J. Schantz *et al.* (*3*), and the puffer-fish poison.

The action of both toxins was tested on Ranvier nodes of a single frog sciatic nerve fiber prepared according to the method of Staempfli (*4*), and on the single isolated electroplax of *Electrophorus electricus*, preparaed with the method developed by Schoffeniels (*5*). In the innervated membrane of the latter cell there are many synaptic junctions, although most of the surface area is conducting membrane; the two types of membranes can be readily distinguished by electrical characteristics.

On exposure of Ranvier nodes to either of the toxins in concentrations of about $3 \times 10^{-10}M$ (10^{-4} μg/ml), electrical activity was rapidly and reversibly blocked within 30 seconds (See Fig. 1). With higher or lower concentrations the period of time required changed correspondingly. When the electroplax was exposed to either of the two toxins, a marked difference in the effect was observed. Clam poison, in a concentration of 5×10^{-4} μg/ml, blocked the response to neural stimulation in 10 to 20 minutes. With higher toxin concentration (0.1 μg/ml) the inhibition occurred in seconds. A concentration of 0.2 μg/ml was required to block the response to direct stimulation. Puffer-fish poison, in a concentration of 0.025 μg/ml, blocked the response to both direct and indirect stimulation simultaneously. The presence of curare (50 μg/ml) did not change the concentrations required for blocking the response to direct stimulation. Inhibition by puffer-fish poison at concentrations of 0.1 to 0.25 μg/ml, recorded with intracellular electrodes, occurred without depolarization.

The extraordinary toxicity of the compounds, which is several orders of magnitude higher than that of the most toxic nerve gases, raises the interesting and challenging problem as to the underlying mechanism. The primary role of the acetylcholine system in the generation of bioelectric potentials prompts the question of whether that system may be affected by these toxins. A reaction of these toxins with any one of the members of the system is thus conceivable. However, the affinity of the toxins to acetylcholinesterase is so low that a reaction with this enzyme may be excluded as a casual factor (*6*). A few tentative tests by S. Ehrenpreis with the receptor were inconclusive; considerable modifications of techniques are required for analysis. Action on the storage protein leading to a release of the ester appears unlikely, since there seems to be no depolarization, and since the blocking effect persists as long as the toxins are present. Obviously, the toxins may react with entirely different constituents of the membrane. At present, no satisfactory explanation can be given of the underlying chemical reaction. But the data reported promise to provide biology with a new potent tool for the analysis of events associated with nerve activity (*7*).

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25 April 1960

Eating or Drinking Elicited by Direct Adrenergic or Cholinergic Stimulation of Hypothalamus

Abstract. A double cannula system, allowing repeated stimulation of central structures with crystalline chemicals, was developed. This technique was employed to study the effects of adrenergic and cholinergic stimulation of the lateral hypothalamus of rats. Drug-specific effects on the feeding and drinking mechanisms, respectively, were observed.

The exploration of the central nervous system by means of electrical stimulation has provided a wealth of information of great interest to physiologists and psychologists alike. The usefulness of this technique is limited, however, because the effects of stimulation are not restricted to synaptic junctions but affect fibers of passage, causing conduction in both normal and antidromic directions.

It has long been recognized that chemical stimulation avoids these problems, but the technique has in the past been plagued by the problem of uncontrolled spread, which raises a serious objection to the injection of chemicals in solution. Attempts to control for this factor by minimizing the injected quantities have apparently not been completely successful in preventing the escape of the fluid along the shank of the needle, following the path of least resistance.

Depositing chemicals in solid form has been shown to reduce this problem greatly (*1*), but this method has not allowed repeated stimulation of a selected locus. In the present study, a technique was developed which avoids this objection.

A double cannula system, consisting of two modified syringe needles, was permanently implanted unilaterally, by means of a stereotaxic instrument, into the lateral hypothalamus of each of 12 albino rats. Histological verification of the intended placements showed the tip of the cannula to be located in a circumscribed perifornical region at the same rostrocaudal coordinate as the ventromedial nucleus (see Fig. 1), an area corresponding to the ventral portion of Anand and Brobeck's "feeding area" of the lateral hypothalamus (*2*).

After 5 days of postoperative recuperation, the inner cannula was removed and minute amounts (1 to 5 μg) of crystalline chemicals were tapped into its tip before it was returned to its usual position. Successive treatments were administered to all animals in a counterbalanced order, with a minimum of 3 days between injections. Both food and water were freely available throughout the experiment. The food and water consumption of satiated rats was re-

corded for 1 hour immediately following stimulation and compared with the consumption in a comparable period immediately preceding the injection. Daily food and water consumption records were maintained.

None of the animals ever consumed food or water in measurable quantities during the prestimulation period. The injection of epinephrine or norepinephrine resulted in highly significant ($p < .01$) food consumption beginning 5 to 10 minutes after stimulation and persisting with variable intensity for 20 to 40 minutes. Food consumption averaged 3.0 gm under epinephrine and 4.3 gm under norepinephrine.

The injection of acetylcholine (capped by physostigmine) or carbachol into the identical loci in the same animals resulted in highly significant drinking ($p < .01$), the latency, duration, and magnitude of the effect being comparable to those obtained for eating after the injection of adrenergic substances. Water consumption averaged 7.4 ml after the injection of acetylcholine and 12.8 ml after the injection of carbachol, this difference being highly significant ($p < .01$). There was no significant food consumption after cholinergic stimulation (see Fig. 2).

The injection of adrenergic substances resulted in significantly less water intake than cholinergic stimulation ($p < .01$). Since in all but one animal the drinking occurred only after a considerable amount of dry food had been consumed, water consumption seemed to be secondary to the food intake rather than a direct consequence of stimulation. To establish further the specificity of the adrenergic effect, norepinephrine was deposited in the lateral hypothalamus of six food- and water-satiated animals, which were then placed in observation cages containing only water. For 30 minutes after the injection none of the animals consumed measurable quantities of water, though

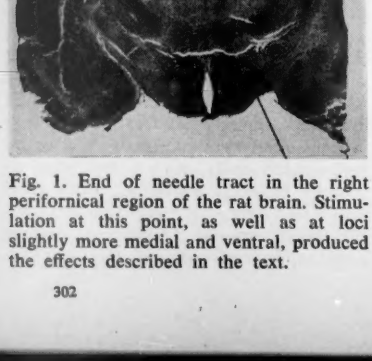

Fig. 1. End of needle tract in the right perifornical region of the rat brain. Stimulation at this point, as well as at loci slightly more medial and ventral, produced the effects described in the text.

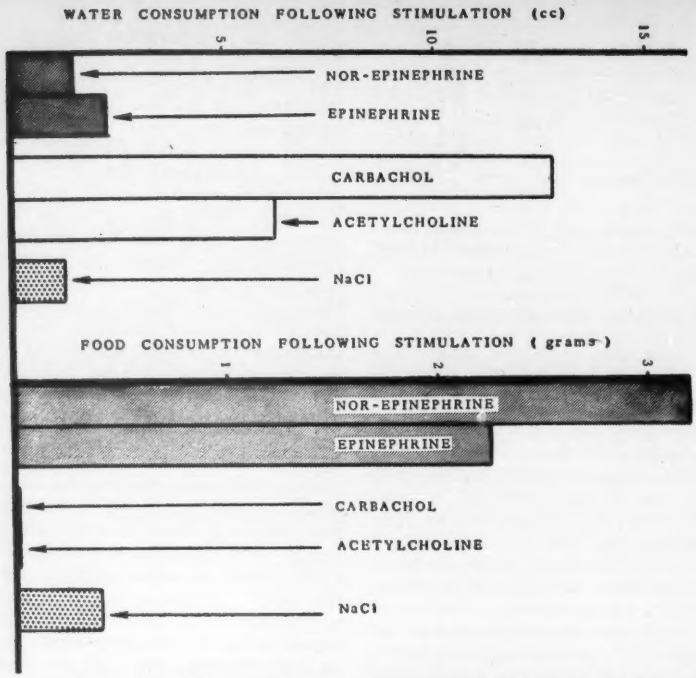

Fig. 2. Food and water intake during 1 hour following stimulation. (The intake during a comparable control period was zero in all cases and is not shown.)

four of them repeatedly sampled the drinking tube very briefly. Food was then introduced, and all animals ate almost immediately, though total food consumption was lower than that normally observed, since the food was introduced only toward the end of the period previously established as the duration of the adrenergic effect.

In order to control for the effect of osmotic stimulation, comparable amounts of NaCl were deposited in all the animals. No significant food or water intake was observed. In order to control for general excitation effects, strychnine in comparable quantities was deposited in six animals which also showed the above-described effects of adrenergic and cholinergic stimulation. No consumatory behavior was observed following this stimulation.

The daily consumption records indicate that the amount of food or water consumed during the 1-hour period after stimulation, totaling as much as 40 percent of the animal's normal daily intake, appeared to be consumed above and beyond the normal daily intake. Because of the variability of these records, no statistical evaluation of this effect can be presented, but the conclusion is supported, at least for eating, by the consistent weight gain observed on the day following adrenergic stimulation.

A control for the specificity of the localization of the observed effects was obtained in a preliminary study designed to yield optimal stereotaxic coordinates for the study reported here. It was found that very small deviations from the optimal position, shown in Fig. 1, sufficed to eliminate the effects completely.

The results of this investigation indicate that (i) cell concentrations active in the regulation of both food and water intake are present in the lateral hypothalamus; (ii) cell concentrations exerting this control appear to be highly localized but not clearly separate from each other, since stimulation of "identical" loci in the same animal can evoke both forms of behavior; and (iii) the feeding mechanism appears to be selectively activated by adrenergic stimulation, while the drinking mechanisms appear to respond selectively to cholinergic stimulation (*3*).

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11 April 1960

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Letters

Social Responsibility of Scientists

The editorial "Between two extremes" [Science **131**, 1013 (8 Apr. 1960)] finds the two extreme attitudes toward the social responsibility of American scientists to be: (i) delegation by the scientists of responsibility for social consequences of his research to the state, and (ii) refusal by the scientist to do the work which his evolving conscience deems a threat to the "country or humanity at large."

The editorial chooses the middle way, the way of free expression by the scientist of his knowledge and considered opinion, while he remains loyal and faithful to the policies of his government. In making this choice, the editorial consigns the second "extreme" to the limbo of "anarchy."

It is my view that the second "extreme" position is in order wherever mass death is planned and carried out, whether under Hitler, Nehru, Eisenhower, Khrushchev, or whomever. Linus Pauling, in his book *No More War!*, states that "testing of one great superbomb, with 10 megatons of fission, requires the sacrifice of . . . 1500 children, . . . 150,000 children or even more." In the light of this, the second "extreme" position makes sense to a great many of us in the United States, England, Russia, France, and elsewhere.

Statements made in the editorial are related to the following quotation from G. B. Kistiakowsky's article, "Science and foreign affairs," in the same issue: "We, as scientists, must do all we can to help keep the tools of our diplomacy and the tools of our force in efficient readiness." The phrase "tools of our force" needs special consideration in the age of weapons of mass annihilation. The editorial mentions the lunch-counter demonstrations in the current moral struggle for human rights in this country. The tools of force used in this struggle are successful insofar as they are not tools of violence. In other words, there appear to be two kinds of ultimate force to be used when diplomacy "fails"—namely, tools of violence and tools of nonviolent force.

I am persuaded that putting our tools of nonviolent force into readiness is more important than readying our tools of violence. The former can eliminate the necessity for the latter. Is there a government program for the development of nonviolent force, in the sense that Martin Luther King or M. K. Gandhi have used the term?

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I do not think that a scientist can shift the moral responsibility for the work he does to someone else. There was a jeweler who made a timing device to explode a bomb in an aeroplane for a man who wished to kill his wife, and necessarily also the other occupants of the plane. The jeweler was adjudged equally guilty with the man who attached the device to the bomb and concealed it in the plane. The scientist who contributes his skill to the development of nuclear or biological agents whose sole use is for the killing of thousands or millions of his fellow human beings would seem to be just as morally responsible as the jeweler, and perhaps several orders of magnitude more so. Neither a morally sensitive jeweler nor a morally sensitive scientist would accept such work.

FORREST F. CLEVELAND
Illinois Institute of Technology,
Chicago

Elementary-School Science

The editorial of 13 May [*Science* **131**, 1405 (1960)], which reviews an article of mine on teaching elementary-school science, contains several points with which I now agree. (The article was written several years ago, although it was not published as part of a book until early 1959.) Obscure and flowery language is a fit subject for criticism. However, I do want to make a strong statement on one substantive issue raised in the editorial and to clarify my present position on a second.

Children can exhibit more than factual recall in elementary-school science and should be encouraged to do so. To begin to view science as a stimulating intellectual endeavor, a child must realize that speculation plays a crucial role in scientific discovery. One way to foster this realization is to create situations in which the youngster himself can (yes) hypothesize, where he can also suggest methods for testing hypotheses. A second-grade youngster certainly can speculate about conditions that speed evaporation. There is no doubt he can suggest rational, even rigorous, methods of putting his speculations to further test. Alerting the teacher to the fact that children can "formulate and suggest tests for hypotheses" in science, as I did in the article, may add a dimension to teaching that places it more in consonance with a modern view of science than is usually true of science instruction at any level.

However, it is a second point that I most wish to clarify. The editorial states the general argument of my chapter as follows: "Even young children should be taught 'to apply the scientific method in solving their everyday academic and personal problems.'" Whether or not this theme is indeed the general argument of the chapter is open to question. Certainly the sentence quoted, as the editorial acknowledges, came not from anything I wrote but from an introductory remark by the editor some 357 pages removed.

More significant is the fact, which I chiefly wish to stress in this letter, that my efforts for the past 2 years have been directed primarily toward demonstrating the merits of a viewpoint directly opposite to that ascribed to me in the editorial. An elementary-school science program wherein content is selected primarily on the basis of personal and social utility is weak on several grounds. In present programs reflecting this bias, elementary-school children often study details of municipal sewage systems because such knowledge helps them better to understand how man makes use of science. For the same reason, they study design of space suits and construction of automobile engines. Criteria for content selection that emphasize the theme of utility lead to patchwork curricula. At their best, such programs lack foundation in basic scientific principles; at their worst, they stress solely the rapidly changing gadgetry confused in the public mind with science.

Some of us at the University of Illinois have been pursuing a program for 2 years in which the choice of content for elementary-school children is made by the professional scientist entirely on the basis of the significance of the content within the discipline. We have been working in astronomy so far. My role in this effort has been to try the topics so selected with youngsters in regular elementary-school classrooms. For example, we have been working with some classes to see whether children can acquire a concept of a geocentric solar system based on evidence that is relatively simple to obtain. As far as I know, the social or personal utility of this idea is nil. Yet the problem thus posed does give children an insight into a significant astronomical problem, albeit an old one. Children begin to understand by such study what astronomers do, what types of evidence they must collect. Similarly, we have been teaching children how distances are measured in space (instead of just telling them the distances that are thus measured, as is usually the case at present).

There are many topics we plan to try. The work so far has been episodic. But on the basic of limited trial we can say that content selected solely because of its crucial role in a scientific discipline is intellectually exciting for children,

and the comprehension of such content by children in the normal intelligence range is truly surprising, particularly in the light of our current expectations of children.

I have written at greater length elsewhere on this theme [see my article in *The Science Teacher* (March 1960)], and I expect to spend the next several years assessing the feasibility of elementary-school science programs based on content selection by professional scientists.

J. MYRON ATKIN
College of Education,
University of Illinois, Urbana

Weather Forecasting

In his recent article "The atmosphere in motion" [*Science* **131**, 1287 (1960)], Robert R. Long has presented an interesting summary of his well-known work on the channel flow of stratified fluids, and the comparisons between theory and experiment which he presents are impressive. I think most dynamic meteorologists would certainly agree with him in stressing the need for a great deal more basic hydrodynamical research in order to strengthen the foundations of dynamical weather prediction. I also feel that he would find few who would quarrel with the statement that forecasting accuracy has improved little in the past 40 years or so, although more variables are now predicted over greater regions of the atmosphere. Long's introductory remarks on the role of numerical weather forecasting in the past decade, however, may be misleading to the general scientific reader and deserve some comment.

The numerical (or dynamic) forecasts now used subjectively by the forecasting meteorologist differ from his other sources of information in at least two important and fundamental respects. In the first place, the numerical forecasts represent the result of a systematic application of dynamical equations to the problem of large-scale atmospheric flow and are in this sense objective and reproducible. Secondly, the numerical forecasts may be (and have been) systematically improved by the introduction of more realistic models and previously neglected physical effects, as well as by improvement of the numerical procedures employed in the solutions. From a practical viewpoint the test of a forecast is, of course, its accuracy, and in this respect the present numerical predictions are disappointing in some ways. The low-level forecasts issued, for example, by the Joint Numerical Weather Prediction Unit in Suitland, Md., are not superior to those produced by the usual synoptic means; the higher-level (500 millibar) numerical forecasts, on the other hand, are now more accurate for periods up to 3 days than other comparable forecasts. This recent improvement has resulted from the systematic error reduction noted above. In view of the many physical and mathematical approximations incorporated in present operational models, I feel that their performance is more surprising than disappointing; relatively simple dynamical methods are here effectively competing with all of the synoptic calculations and intuitive skill of the forecaster.

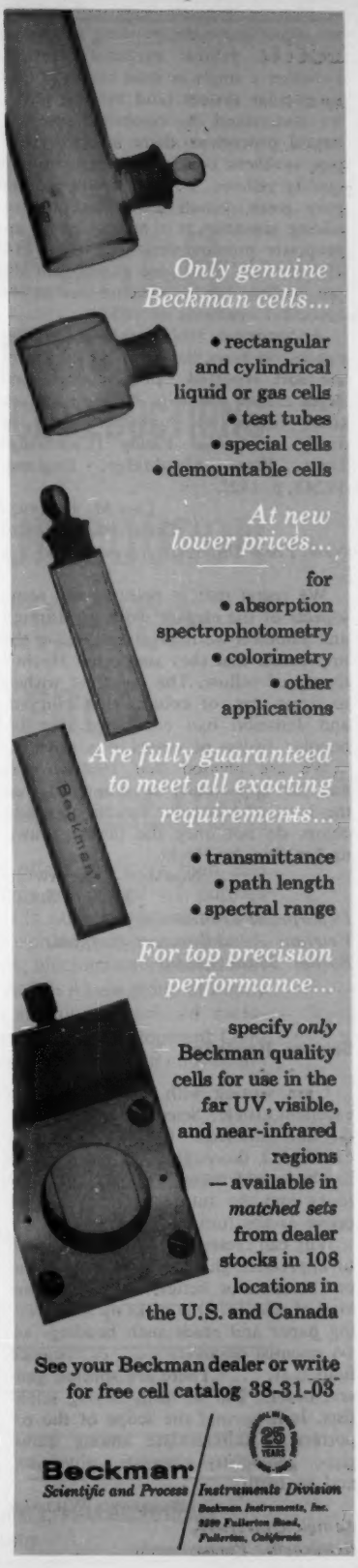
From a broader viewpoint, the numerical integrations represent an attempt to verify the same set of basic dynamical equations with which Long is concerned, although for larger-scale phenomena, in which different physical effects are important. While the comparison of theory and observation is here poorer than in the more restricted experiments of Long, I feel there is good reason to entertain more optimism than he suggests is in order. The small but systematic improvement in the prediction of the large-scale flow is here, I believe, a significant improvement. As this scale of motion is progressively better understood, the results of research on small-scale phenomena—of which Long's studies of tornado-like circulations is an excellent example—may then be incorporated into the overall dynamical picture and should result in further systematic forecast improvement, especially for the smaller-scale motions which are closely associated with our subjective impressions of "weather."

W. LAWRENCE GATES
Department of Meteorology,
University of California, Los Angeles

Binocular Fusion of Colors

In an article entitled "Colors of all hues from binocular mixing of two colors" that appeared in *Science* [**131**, 608 (1960)], the following statement was made by Geschwind and Segal. "The problem of binocular fusion of colors has interested investigators since Hecht's demonstration in 1928 that presenting red to one eye and green to the other led to a subjective sensation of yellow. . . . Hurvich and Jameson . . . confirmed these results; it is today generally accepted that such fusion is readily obtainable in most subjects."

A major finding of the article by Hurvich and Jameson cited by Geschwind and Segal was the following: "The fact that does clearly emerge from these results is that, unless there is a yellow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sensation and corresponding to it some form of yellow excitatory process (whether a single or dual event) in each monocular system (and by that phrase we understand the combined receptor-neural processes), there is no mysterious, synthetic central emergence of the quality yellow. . . . When pure red and pure green stimuli are mixed, the resulting sensation is a neutral at an appropriate mixture ratio" [*Science* **114**, 199 (1951)]. Thus, our experiment did not confirm Hecht's conclusions on the binocular synthesis of yellow.

A historical bibliography of earlier papers on binocular contrast effects of the sort reported by Geschwind and Segal can be found in Parson's text on color vision [*An Introduction to the Study of Colour Vision* (Cambridge Univ. Press, Cambridge, England, 1924), p. 142].

LEO M. HURVICH
DOROTHEA JAMESON
New York University, New York

We regret that in pointing out some aspects of the elegant work of Hurvich and Jameson we inadvertently gave the impression that they supported Hecht's theory of yellow. The point we wished to stress was, of course, that Hurvich and Jameson had confirmed that binocular fusion of colors was possible.

We are pleased that Hurvich has had the opportunity to point out that the laws of binocular fusion of single colors do not obey the theory drawn up for them by Hecht.

NORMAN GESCHWIND
JOHN R. SEGAL
Department of Neurology,
Veterans Administration Hospital,
Boston, Massachusetts

Science Reporting

I am writing with reference to the excellent article "Science reporting—today and tomorrow" [*Science* **131**, 1193 (1960)]. I thoroughly commend what has been said therein. Since the author looks into the future, let's try to improve as the future rolls around.

Why can't science reporters be trained to appreciate that there is more than one science, or better, more than one kind of science? One picks up the morning paper and reads such headings as, "A scientist discovers. . . ." or "Science finds that. . . ." There are species, genera, orders, and so forth among scientists. Is it beyond the scope of the reporters to differentiate among biologists, geologists, chemists, physicists, and so forth?

BRADFORD WILLARD
Lehigh University,
Bethlehem, Pennsylvania

Meetings

Free Radicals in Biological Systems

A symposium on free radicals in biological systems was held at Stanford University from 21 to 23 March under the joint sponsorship of the Biophysics Laboratory of Stanford University and the Biophysics and Biophysical Chemistry Study Section. Support was derived from the study section's special programing funds (grant RG-5048) from the National Institutes of Health, U.S. Public Health Service. The symposium was attended by 75 invited participants and included 30 papers summarizing much of the current research in this rapidly developing field.

Britton Chance (University of Pennsylvania) gave the opening paper, on free radicals in enzyme substrate compounds, and reviewed the prolific work of Leonor Michaelis, which has been responsible for much of the current interest in free radicals as naturally occurring biochemical intermediates.

Recent research on biological free radicals by other than magnetic resonance methods was discussed by H. Beinert (Wisconsin), who reported on studies of semiquinone formation by flavins and flavoproteins conducted by optical absorption spectroscopy, and by A. S. Brill (Cornell), who discussed the detection of free radical intermediates by use of a magnetic susceptibility balance incorporating a flow system. V. Massey (Sheffield) spoke on intermediates in the lipoyl dehydrogenase-substrate system as observed by optical spectroscopy.

George Pake (Stanford) discussed the general problem of applying electron paramagnetic resonance methods to the study of biological and biochemical systems and described specific approaches (sample-holder design, increase of modulation frequency, and so on) to the problems inherent in observing free radicals in aqueous, conducting systems. Electron paramagnetic resonance instrumentation was further discussed by B. Chance (Pennsylvania), who reported on a rapid-flow apparatus for spin resonance measurements, and by A. Müller (Radio-biology Institute, Karlsruhe, Germany), who described a double-cavity method for precision measurements of free radical concentration.

M. S. Blois (Stanford) reported on a series of precise g-value measurements of free radicals in solution and discussed the interpretation of g values in terms of free radical structure. M. W. Hanna (California Institute of Technology) described recent work done on the paramagnetic resonance of long polyene radicals. H. Beinert reported on paramagnetic resonance observations of semiquinone formation by flavins and flavoproteins—observations made with R. Sands (Michigan) as a parallel study to his optical measurements. L. Augustine (Atomic Energy Commission) discussed the thermoluminescence of irradiated biochemicals.

An attempted demonstration of free radical intermediates in reactions catalyzed by pyridinoproteins was discussed by H. Mahler (Indiana), and T. Nakamura (Pennsylvania) described the results of his paramagnetic resonance observations on free radicals in enzymatic oxidations. The free radical intermediates which appear during the autoxidation of dihydroxyphenylalanine were discussed by J. E. Wertz (Minnesota); L. H. Piette (Varian Associates, Palo Alto) and I. Yamazaki (Oregon) spoke on the identification of free radical intermediates during the course of the peroxidase-substrate reaction. T. Vänngard (Uppsala) described his research on the free radicals and metal valency changes occurring in the xanthine oxidose-substrate systems.

B. T. Allen (North Staffordshire) reported on some of the recent work in Ingram's laboratory and discussed the production of unpaired electrons in large molecules by ultraviolet irradiation. D. H. Whiffen (National Physical Laboratory, Teddington) spoke on the paramagnetic resonance spectra of the free radicals in irradiated single crystals of glycine and glycollic acid, and the spectra of several irradiated peptides were described by H. D. Box (Roswell Park Memorial Institute). M. L. Randolph (Oak Ridge National Laboratory) reported on his studies of the decay rates of radiation-induced free spins in crystalline amino acids, and Walter Gordy (Duke) discussed his paramagnetic resonance studies of cytochrome and hemoglobin. A paper by T. Henriksen (Norsk Hydro, Oslo) on the free radicals of irradiated thiols and disulfides was read by Tor Brustad, currently at Berkeley. Radicals and radiation damage in irradiated choline chloride were discussed by R. O. Lindblom (Dow Chemical Co., Pittsburg, Calif.)

Free radicals in systems of greater complexity were next considered. D. E. Smith (Argonne National Laboratory) spoke on free radicals in photodynamic systems, and P. B. Sogo described the paramagnetic resonance studies of photosynthetic materials carried out in Calvin's laboratory. Bernard Smaller (Argonne) discussed his findings on photo-induced free spin species in plant pigments. A. Müller read a paper describing the work in Zimmers' laboratory (Radiobiological Institute) on radiation-produced free radicals in biological systems, and Anders Ehrenberg (Caroline Institute) continued with

further discussion on radiation-produced radicals in living and dead materials. E. L. Powers (Argonne) gave a concluding paper on the role of free radicals in the lethal effect of x-rays on dry bacterial spores.

The symposium was a unique, congenial, and timely meeting, devoted to a subject matter of rapidly growing interest. The proceedings will be published by Academic Press and should appear later this year.

M. S. BLOIS
Biophysics Laboratory, Stanford University, Stanford, California

Forthcoming Events

August

21–6. Pacific Science Cong., 10th, Honolulu, Hawaii. (Secretary-General, 10th Pacific Science Cong., Bishop Museum, Honolulu 17)

22–25. American Astronomical Soc., Mexico City, Mexico. (J. A. Hynek, Smithsonian Astrophysical Observatory, 60 Garden St., Cambridge 38, Mass.)

22–25. American Physiological Soc., San Francisco, Calif. (R. G. Daggs, APS, 9650 Wisconsin Ave., NW, Washington 14)

22–26. Plasma Physics, symp., Gatlinburg, Tenn. (University Relations Div., Oak Ridge, Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

22–26. Western Resources, 2nd annual conf., Boulder, Colo. (M. E. Garnsey, Dept. of Economics, Univ. of Colorado, Boulder)

23–25. Assoc. for Computing Machinery, natl., Milwaukee, Wis. (J. Moshman, ACM, Council for Economic and Industry Research, 1200 Jefferson Davis Highway, Arlington 2, Va.)

23–25. Cryogenic Engineering Conf., Boulder, Colo. (K. D. Timmerhaus, CEC, Dept. of Chemical Engineering, Univ. of Colorado, Boulder)

23–26. American Statistical Assoc., annual, Palo Alto, Calif. (D. C. Riley, ASA, Beacon Bldg., 1757 K St., NW, Washington 6)

23–26. Biological Photographic Assoc., Salt Lake City, Utah. (Miss J. H. Waters, Box 1668, Grand Central Post Office, New York 17)

23-26. Institute of Mathematical Statistics, annual, Stanford, Calif. (W. Kruskal, Dept. of Statistics, Eckhart Hall, Univ. of Chicago, Chicago 37, Ill.)

23–28. American Ornithologists' Union, Ann Arbor, Mich. (H. G. Diegnan, Division of Birds, U.S. National Museum, Washington 25)

24–27. Forest Biology Conf., Seattle, Wash. (Miss E. N. Wark, Technical Assoc. of the Pulp and Paper Industry, 360 Lexington Ave., New York 17)

24–27. Internal Medicine, 6th intern. cong., Basel, Switzerland. (Secretariat, 6th ICIM, 13 Steinentorstre, Basel)

24–2. International Union for the History and Philosophy of Science, Stanford, Calif. (R. Taton, 64, rue Gay-Lussac, Paris 5°, France)

25–27. Chemical Organization of Cells,

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2nd conf., Madison, Wis. (J. F. A. McManus, Dept. of Pathology, Univ. of Alabama Medical Center, Birmingham)

25–3. High Energy Nuclear Physics, intern. conf., Rochester, N.Y. (W. A. Jamison, Dept. of Physics and Astronomy, Univ. of Rochester, Rochester 20)

27–30. International Union of Biological Sciences, section of embryology, Pallanza, Italy. (F. E. Lehmann, Kuhnweg 10, Berne, Switzerland)

28–31. American Phytopathological Soc., Green Lake, Wis. (W. B. Hewitt, Dept. of Plant Pathology, Univ. of California, Davis)

28–31. Potato Assoc. of America, Green Lake, Wis. (R. L. Sawyer, Long Island Vegetable Research Farm, Cornell Univ., Riverhead, N.Y.)

28–31. Soil Conservation Soc. of America, Guelph, Ontario, Canada. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa)

28–1. American Inst. of Biological Sciences, annual, Norman, Okla. (H. T. Cox, AIBS, 2000 P St., NW, Washington 6)

28–1. Association of American Geographers, East Lansing, Mich. (M. F. Burrill, Office of Geography, Dept. of Interior, Washington 25)

28–1. Diseases of the Chest, intern. cong., Vienna, Austria. (M. Kornfeld, 112 E. Chestnut St., Chicago 11, Ill.)

28–2. Combustion, 8th intern. symp., Pasadena, Calif. (Office of Industrial Associates, California Inst. of Technology, Pasadena)

28–2. International Pharmaceutical Federation, Copenhagen, Denmark. (A. W. Tønnesen, Bispebjerg Hospital, Copenhagen, N.V.)

28–2. International Soc. for the Welfare of Cripples, world cong., New York, N.Y. (D. V. Wilson, 701 First Ave., New York 17)

28–3. Electron Microscopy, European regional conf., Delft, Netherlands. (A. L. Housink, Lab. v. Microbiologie, Julianalaan 67A, Delft)

28–3. Histochemistry and Cytochemistry, 1st intern. cong., Paris, France. (R. Wegmann, Institut d'Histochimie Medicale, 45, rue des Saints-Pères, Paris 6°)

29–31. American Sociological Assoc., New York, N.Y. (D. R. Young, Russell Sage Foundation, 505 Park Ave., New York)

29–31. Clinical Chemists (Canadian and American Societies), annual, Montreal, Canada. (E. Harpur, Montreal Children's Hospital, Montreal)

29–31. Electron Microscope Soc. of America, 18th annual, Milwaukee, Wis. (W. C. Bigelow, Dept. of Chemical and Metallurgical Engineering, Univ. of Michigan, Ann Arbor)

29–31. Metallurgy of Elemental and Compound Semiconductors, Boston, Mass. (E. O. Kirkendall, AIME, 29 W. 39 St., New York 18)

29–31. Water Quality Measurement and Instrumentation, PHS symp., Cincinnati, Ohio. (R. T. Hyde, Robert A. Taft Sanitary Engineering Center, 4676 Columbia Parkway, Cincinnati 26)

29–1. Ballistic Missile and Space Technology, 5th symp., Los Angeles, Calif. (C. T. Morrow, Space Technology Laboratories, P.O. Box 95001, Los Angeles 45)

(See issue of 22 July for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ DIFFERENTIAL-PRESSURE METER is said to be capable of resolution of 0.05 percent. Interchangeable capsules are available to measure full-scale ranges of ±1, 3, 10, 30, and 100 in. of water with an overpressure capability of 30 lb/in.^{2} Pressure measurements are directly indicated and are provided as voltage analog with ±10 volts full scale for all ranges. Capsules are made of stainless steel. (Decker Corp., Dept. Sci665, 45 Monument Rd., Bala Cynwyd, Pa.)

■ PORTABLE HARDNESS TESTER applies a minor load of 10 kg and a major load up to 150 kg. Readings are direct in Rockwell A, B, or C scales. Electromagnetic or chain clamps are used to apply the instrument to the object tested. Curved as well as flat surfaces can be tested. Standard equipment includes a cone diamond penetrator, a ball penetrator, a standard C-clamp, anvils, and test block. (Steel City Testing Machines, Dept. Sci667, 8817 Lyndon Ave., Detroit 38, Mich.)

■ SPECIFIC GRAVITY INSTRUMENT indicates directly and continuously specific gravity of flowing liquids. The density-sensing device is a totally submerged, chain-weighted glass plummet. Accuracy is said to be ± 1 percent of full scale. Any 0.05 or 0.10 specific gravity span within the limits 0.65 to 1.40 can be selected. (Precision Thermometer and Instrument Co., Dept. Sci669, 1434 Brandywine St., Philadelphia 30, Pa.)

■ BIOLOGICAL MICROSCOPES of inverted type are available in laboratory and research models, either monocular or binocular, with bright-field or phase-contrast optics. Magnifications up to 2000 times are provided. Illuminators are built in, and a wide choice of accessories includes a long working-distance, 40× objective, and a number of cameras. (Unitron, Dept. Sci671, 204 Milk St., Boston 9, Mass.)

■ VACUUM THERMOCOUPLE JUNCTIONS, for electrical measurements from d-c to 10 Mcy/sec, provide sensitivities from 2.5 to 1000 ma full scale. Thermocouples are in thermal contact with the current-carrying heater, but electrically insulated from it for 100 volts d-c. Open-circuit voltage and junction resistance are individually calibrated to ±0.5 percent. (Winslow Co., Dept. Sci675, 701 Lehigh Ave., Union, N.J.)

■ GAS FRACTION COLLECTOR permits quantitative collection of individual radioactive organic components in a gas chromatography effluent stream. A turntable 8 in. in diameter holds up to 50 cartridges packed with silicone-coated scintillation crystals. Vapors leaving the chromatograph detector pass through a heated gas-injection nozzle and condense on the coated surfaces. The operator controls change of cartridges in accordance with observation of chromatogram peaks. Counting efficiency for carbon-14 in the cartridges is said to be greater than 50 percent. (Packard Instrument Co., Dept. Sci668, P.O. Box 428, La Grange, Ill.)

■ RADIATION THERMOMETER uses a mirror system to focus radiation onto an infrared detector. Output of the detector is amplified and presented on a meter or other output indicator. Time constant is said to be 0.002 sec for continuous measurement and 10 μsec for transient measurements. Temperature range is 100° to 8000°F. Focusing range is 4 ft to infinity and resolution angle is 0.5 deg. (Radiation Electronics Co., Dept. Sci679, 5600 Jarvis Ave., Chicago 48, Ill.)

■ RADIANT ENERGY HEAT LAMP is designed to provide high temperatures for x-ray diffraction, hot-stage microscopy, mechanical testing, and the like. It is said to be capable of heating a specimen to 900°C without mechanical or electrical interaction. (Materials Research Corp., Dept. Sci688, 47 Buena Vista Ave., Yonkers, N.Y.)

■ CAPACITANCE BRIDGE is designed for application to noncontact capacitive gaging. The instrument is transistorized and battery-powered. Frequency response is 12 kcy/sec. Provision is made for 60 and 400 cy/sec modulation of the bridge carrier. Sensitivity is 3.5 volt/pf with 10-ft cable and 0.9 volt/pf with 30-ft cable. Dimensions are 6 by 4.5 by 3.5 in. (Decker Corp., Dept. Sci676, 45 Monument Rd., Bala Cynwyd, Pa.)

■ CHLORIDE-ION ANALYZER continuously and automatically monitors Cl^- in aqueous solutions in three standard ranges: 0.1 to 10, 1 to 1000, or 10 to 10,000 parts per million. Measured values may be read directly or may be recorded. The instrument uses a silver–silver chloride electrode and is compensated automatically for temperature effects from 0 to 100°C. Accuracy is said to be ±5 percent of total Cl^- in most applications. (Beckman Scientific and Process Instruments Division, Dept. Sci677, Fullerton, Calif.)

JOSHUA STERN

National Bureau of Standards, Washington, D.C.

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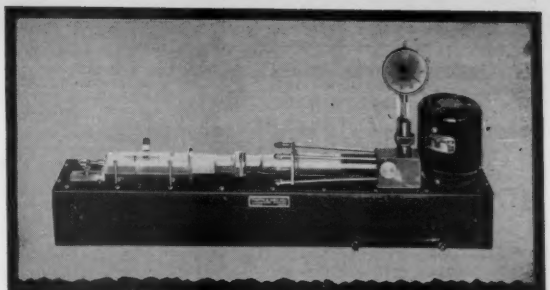

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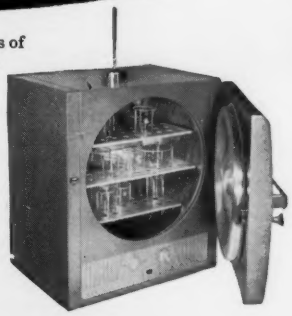

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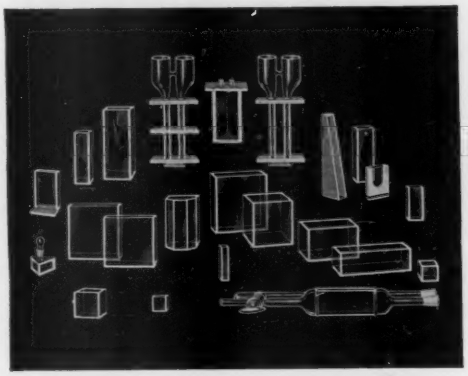

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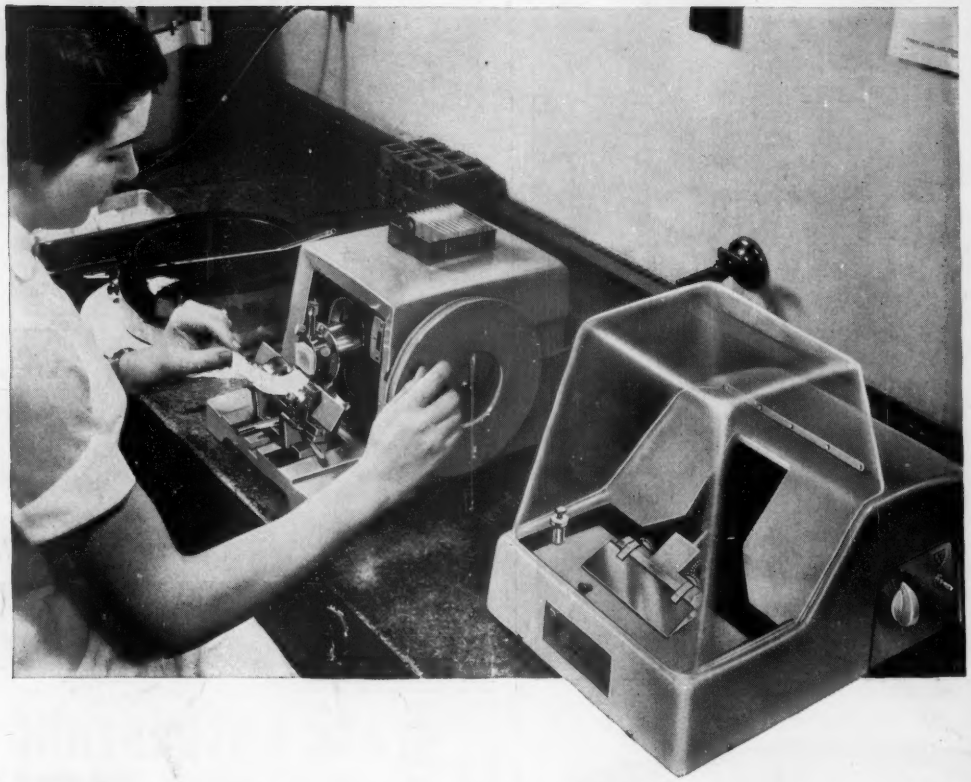

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